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VOLUME 80
JANUARY TO JUNE

PUBLISHED BY
AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
1515 MASSACHUSETTS AVENUE, NW WASHINGTON 5, D. C.
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Cover: A Flame

[*Courtesy National Bureau of Standards, see page 9*]

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THE SCIENTIFIC MONTHLY, established in 1872 as *The Popular Science Monthly*, has been an official publication of the American Association for the Advancement of Science since 1915. It is published for the Association at Business Press, Inc., Lancaster, Pa.

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Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Photographer's Device

A new device called the "Wizard 3-D Close-Up Bar" is designed to simplify the process of taking 3-D close-up pictures. The instrument has four scales, two for use with stereo cameras and two for use with single-lens cameras. The scales are calibrated for distances of 3 to 30 in. The device is designed to eliminate the need for calculations when aligning a camera for the two exposures. (Wizard 3-D Co., Dept. SM, 714 Savings and Loan Bldg., Des Moines, Iowa.)

Easel

Rol-Ease-L has two 20- by 27-in. panels and two metal spring clips for each panel. It takes two rolls of paper and has two guide bars, one of which acts as a paper cutter. Four feet high, the unit is hinged at the top, finished in baked enamel, and steadied by a side-locking brace. The steel legs have rubber tips and support two adjustable paint trays. (Cascade School Supplies, Inc., Dept. SM, North Adams, Mass.)

Transistor Radio

Four transistors, one germanium diode, and one miniature battery are used in the circuit of a new pocket-sized tubeless radio. One transistor is used as an oscillator-mixer, two as IF amplifiers, and one as an AF amplifier. The radio weighs less than 12 oz and measures 3 by 5 by 1¼ in. (Regency Div., Industrial Development Engineering Assoc., Inc., Dept. SM, 7900 Pendleton Pike, Indianapolis, Ind.)

Block and Tackle

Made of aluminum, the Husky Hoist weighs 42 oz but lifts 3000 lb. It is equipped with ball-bearing pulleys, 100 ft of 1000-lb test nylon rope, straps, cadmium-plated rings, and canvas carrying bag. (M & B Sales Co., Dept. SM, 8211 Cedar Springs Ave., Dallas, Tex.)

Cork-to-Glass Cement

Cork-Grip is a new adhesive designed for cementing cork to glass and makes it possible to use glass stoppers as primary seals in decanter-type liquor bottles or to cement cork to glass novelties. (Adhesive Products Corp., Dept. SM, 1660 Boone Ave., New York 60.)

One-Gallon Blendor

A new Waring Blendor of 1-gal capacity has five speeds from 8000 to 16,000 rev/min. It is operated with automatic push-button controls, has a stainless steel container with handle, a removable blade assembly, and a two-section cover with snap-on latches. (Central Scientific Co., Dept. SM, 1700 Irving Park Rd., Chicago, Ill.)

Pocket-sized Tape Recorder

The Midgetape Recorder, 8½ in. long, 1⅞ in. deep, and 3⅞ in. wide, weighs 3¼ lb and is battery-operated and cartridge-loaded. The tape is wound inside the cartridge and no tape threading is required. The recorder, which has three controls, records for 1 hr and simultaneously erases old material as new recordings are made. Battery life is 45 hr. (Mohawk Business Machines Corp., Dept. SM, 944 Halsey St., Brooklyn 33, N.Y.)

Model Oil Refinery Kit

A new model oil refinery kit, the second in a series of three on the petroleum industry, contains plans and materials for building a scale model of a refinery and performing six demonstrations of oil-refining principles. The materials are preformed; over-all size is 22 by 30 in. The kit also contains a diagram showing the flow of oil through a refinery and a 64-page booklet, "The story of oil." The six demonstrations include simple distillation, dewaxing of lubricating oil, vacuum distillation, the use of additives, viscosity, and grease-making. (Models of Industry, Inc., Dept. SM, 2100 5th St., Berkeley 2, Calif.)

Tissue Receptacle

A new semiexpendable tissue receptacle (Fig. 1) is available to users of the Autotechnicon. The bottom of the receptacle is molded of Bakelite. The lid, a paperboard disk that also supports the tissue during its processing run, snaps into a groove on the rim of the bottom. Multiple holes in both lid and bottom allow free circulation of processing fluids during immersion and rapid drainage after removal. Identification can be penciled on top of the lid. Cardboard dividers and auxiliary biopsy liners are also available. (Technicon Co., Dept. SM, 215 E. 149 St., New York 51.)

Fig. 1.

SUGGESTIONS FOR CONTRIBUTORS

These suggestions, if followed by authors, will serve to expedite the reviewing and processing of articles and to reduce appreciably the costs of processing and printing.

Every article, letter, or book review is accepted with the understanding that it has not been published or accepted for publication elsewhere. (Occasionally an important article is reprinted from another periodical, usually one not readily available to readers of *The Scientific Monthly*, but this is always done by special arrangement with the author.)

An article should receive a thorough review before submission, if possible by someone other than the author.

The manuscript should be submitted in duplicate. For the first copy, use a good grade of 8.5- by 11-in. nontransparent paper. *All copy, including quotations, footnotes, tables, literature references, and legends for figures, should be double-spaced.* Leave margins of at least 1.5 in. at the sides and at the top and bottom.

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References and Notes

1. This article is based on a paper given in the symposium, *Species That Feed Mankind*, AAAS meeting, Boston, Mass., 27 Dec. 1953. The studies described were supported by a grant from ..... Acknowledgment is due to John Doe and Helen Blank for assistance in conducting the experiments.
2. W. C. Allee, *Science* **97**, 517 (1943).
3. The two sets of observations represent .....
4. A. K. Goodwon, *An Introduction to Ethics* (Myer Press, New York, ed. 2, 1949), p. 124.
5. This dimensional theory is more fully described in .....

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THE SCIENTIFIC MONTHLY

JANUARY 1955

Flames

HERBERT P. BROIDA and HAROLD J. MOROWITZ

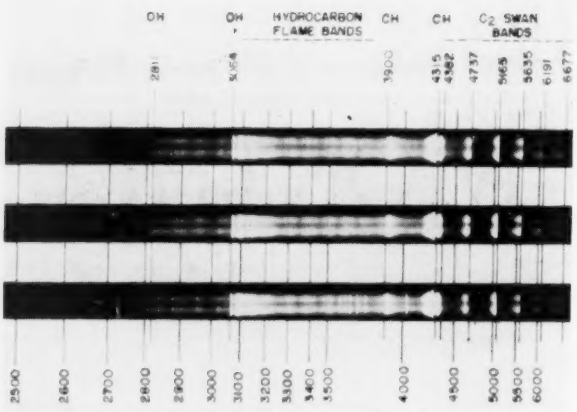
Dr. Broida is a physicist at the National Bureau of Standards, where he has been investigating certain aspects of high-temperature physics. During 1952–53 he was a Guggenheim fellow. He received his training at the University of Colorado and at Harvard University, where he was a teaching fellow and research assistant. He taught physics at Wesleyan University in 1944–45. Dr. Morowitz, who is now in the Laboratory of Technical Development of the National Heart Institute, the National Institutes of Health, was in the Heat and Power Division of the National Bureau of Standards 1951–53. He was trained at Yale University. During 1951–54 he was chairman of the national committee on biological and medical instrumentation, Instrument Society of America.

THE study of flames provides a rare opportunity to combine the rigors of a scientific approach with the fascination that people feel while watching fires. The awesome aspect of flames, which all of us have felt while gazing at burning logs in a fireplace, may have played a large part in the first observation and study of flames by our ancestors in the dawn of man's history.

Reverence toward flames, whether inspired by the terrifying forest blaze or by the comforting hearth fire, led many primitive cultural groups to give fire an important place among their religious beliefs. This same feeling may have persisted in the minds of the Ionian philosophers who elevated fire to a fundamental part of reality in their system of earth, air, fire, and water. And the alchemist, whose furnace was often the center of his laboratory, looked to fire for aid in his cherished task of transmuting the elements. His essentially empirical approach was deeply influenced by the same awe of fire felt by his prehistoric ancestors, an awe that may still be present to a degree in the minds of many contemporary scientists studying flames.

The transition from the alchemical to the chemical point of view is seen in the experimental study of combustion begun in 1660 by the Oxford School of Chemistry under the leadership of Robert Boyle, who is best remembered for his study of pressure-volume relationships in gases. However, the ideas of the early Oxford School were soon forgotten in the enthusiastic acceptance of the "phlogiston theory" of combustion, which maintained that a substance of negative weight escaped from burning objects. This theory was popularly accepted until late in the 18th century, when it was demonstrated to be unnecessary by the brilliant quantitative experiments of the French savant Antoine Lavoisier. Lavoisier established the concepts of Newtonian physics in the chemistry of his day and thus stands as a forerunner of modern students of flames who draw their material with equal utility from both chemistry and physics.

Combustion research formed a significant part of the experimental background of the formulation



Photographic spectra of three petroleum fuels burning in an experimental ram-jet engine. Top, unleaded marine gasoline; middle, grade JP-4 fuel; bottom, grade 100/130 fuel. The spectra show the presence of excited radicals of OH, CH, C_2, and HCO. The bottom spectrum shows spectral lines of atomic lead.

of the modern atomic theory early in the 19th century by the English scientist John Dalton. The combining ratios of oxygen and various fuel gases demonstrated the laws of combining proportions that were so essential to the formulation of this theory.

The researches of Humphry Davy stand out as a landmark in the history of flame studies. His invention of the miner's safety lamp in 1819 pointed out the practical potentialities inherent in the basic research that was being carried out and foreshadowed the tremendous utilitarian application of flame investigations following the industrial revolution. Davy made the first recorded attempt to measure the temperature of a flame, and he was probably the first scientist to study catalytic combustion.

Combustion studies in the period from 1840 to 1880 were greatly influenced by the German chemist R. Bunsen, who finally established quantitative gas chemistry. He was responsible for the bunsen burner that is so well known to students of chemistry, and in 1857 he made the first recorded measurement of the speed with which a flame travels into an unburned mixture of gases. In the 1880's the French investigators E. Mallard and H. Le Châtelier extended the methods of studying flame propagation by applying photographic techniques to increase the precision of measurement. In addition, they formulated a theory to relate quantitatively the flame speed to other measurable variables of the gas mixture.

On the theoretical side, the mid-19th century witnessed in the broader fields of chemistry and physics the establishment and growth of thermodynamics, the science of heat and energy, and the kinetic theory of gases. These advances play important parts in present-day understanding of combustion.

Another tool for these studies has been in the process of development since the early observation by Newton that white light is separated into its component colors when passed through a glass prism. From this beginning the science of spectroscopy has grown to enable investigators to deduce many facts about a light source from an analysis of its spectrum. After 1850 the study of the spectra from flames and from the sun led to G. R. Kirchhoff's clear formulation of the principle that spectra are characteristic of the atoms and molecules that produce them. Stated differently, the color of a flame depends on the atoms and molecules in the flame and the physical state in which they exist.

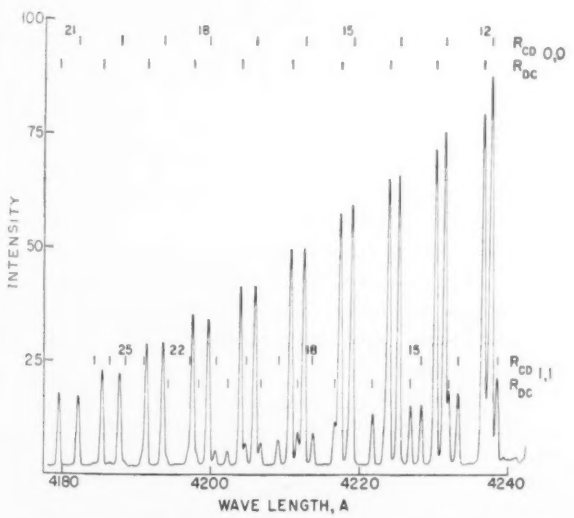
The revolution in scientific thought that characterized the beginning of the 20th century provided the student of flames with new experimental tools and theoretical methods for analyzing his results. The development of precise, reliable, high-resolution spectrometers facilitated the obtaining of data. The advances in the understanding of matter and radiation highlighted by Planck's quantum theory in 1901, Bohr's theory of the hydrogen atom in 1913, and the extensive development of quantum mechanics associated with the names of De Broglie, Heisenberg, Schrödinger, and others in the mid-1920's provided a systematic and highly esthetic scheme for explaining and interpreting the spectroscopic results. The pioneer work of the Swedish chemist Arrhenius stimulated the development of chemical kinetics, the study of the rates at which atoms and molecules combine and dissociate under varying conditions. These ideas have aided in a better understanding of what happens in a flame, and they are necessary for interpretation of complex flame phenomena.

The prime question, "what is a flame?" serves to introduce the difficulties that have led to extensive research and still intrigue the student of combustion. To the Boy Scout starting a campfire and to the housewife lighting a gas stove this question poses little difficulty, and the answer, "a body of burning gas or vapor," offered by Webster's *Collegiate Dictionary* suffices. But when we consider a chemical engineer trying to regulate combustion in a flow process or a design engineer trying to decide what materials to use in building a jet engine, the question takes on aspects that are not answered by this definition.

This article is concerned with attempts to investigate the detailed chemical and physical processes that determine the nature and uses of a given flame.

Therefore instead of asking the general question "what is a flame?" we shall turn to the following specific questions: What substances undergo reactions in flames and what are the final products formed? How is a flame started? In what states are atoms and molecules found in flames? What reactions take place? How are these related to the size, color, temperature, and speed of flames? What is the temperature in various parts of the flame, and what is the meaning of temperature in a flame? How can flames be controlled by controlling the reactants, the pressures, and other variables that the experimenters and practical users have at their disposal?

From a practical point of view knowledge of flames is tremendously important in modern methods of obtaining heat and power. Gas, oil, and coal furnaces are used to heat our buildings, run our generators, and power a large number of industrial processes. Our transportation systems depend on steam and internal combustion engines and more recently upon jet engines. In all these applications it is important to obtain the most energy from fuels by controlling the conditions under which they burn. Effective control ultimately depends on our basic knowledge of the processes that take place. These basic facts are needed by the engineer to control temperature, to control the end-products, to control the speed of burning, and therefore to control the industrial costs and the uses to which combustion may be applied. Another reason for the study of flames is the desire for knowledge, for its own sake, by those individuals who obtain esthetic satisfaction from forming a clear and simple pattern of many apparently unrelated phenomena.



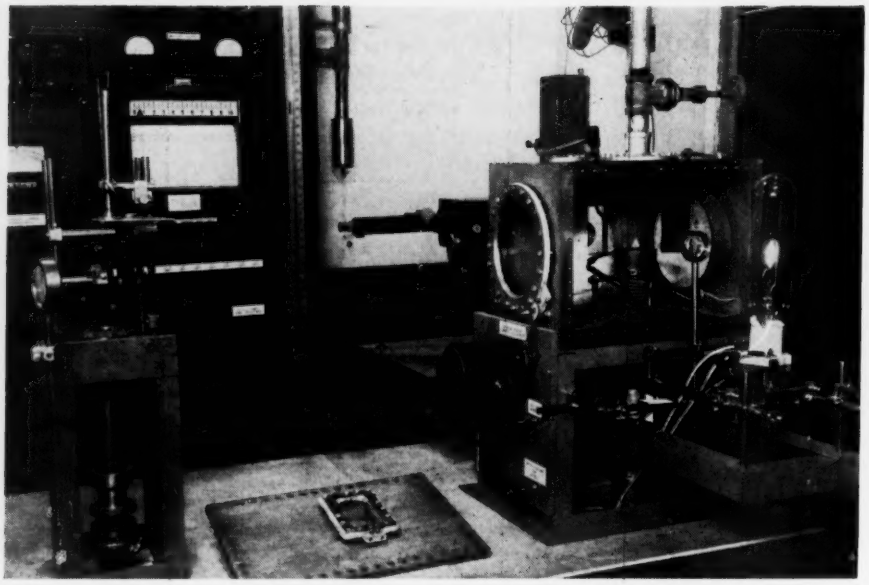
High-resolution spectrum of the radical CH in an oxygen-acetylene welding burner. This spectrum corresponds to an enlargement of a small section of the 4315-A band of CH shown in the photographic spectra on page 4. The relative intensities of the individual spectral lines are used to measure temperature. In this case the temperature is 2725°K (2452°C).

The philosophy with which the contemporary study of flames is approached is fundamental to modern physics and chemistry: study the simplest phenomena first and build up the understanding of complex phenomena from the facts learned in simple cases. This means studying simple flames from mixtures of fuels such as hydrogen (H_2), methane (CH_4), carbon monoxide (CO), or acetylene (C_2H_2) with oxygen (O_2) or fluorine (F_2) in controlled atmospheres of nitrogen (N_2) or argon (A), rather than studying complex flames from organic mixtures such as wood or petroleum. This article is therefore concerned with four measurable quantities of these simple flames: (i) radiation, (ii) temperature, (iii) flame speed, and (iv) reactants and products.

The most familiar type of simple flame is that observed above a properly adjusted laboratory bunsen burner. The fuel gas in this case is premixed with air so that oxygen is more readily available for combustion. The burning zone is a bright luminous region, conically shaped, and usually blue in color. This zone is usually a few tenths of a millimeter thick, but under conditions of low pressures such as those that exist at very high altitudes it can be as thick as several millimeters. The hot gases remain visible above the burning zone, but the radiation emitted is less intense. Surrounding these hot gases there is, in general, a more luminous mantel in which the gases not burned in the primary burning zone undergo complete combustion upon mixing with the surrounding atmosphere.

One of the most intriguing and as-yet-unsolved problems of combustion is the cause of the familiar blue color of the hot gases in flames. Under some conditions the apparent blue seems to contain components of the whole visible spectrum, while under other conditions it has been attributed to radiation from short-lived carbon hydrogen (CH) particles. These particles are intermediates in chemical reactions and exist for such short times that they cannot be detected chemically. They may, however, be identified from their radiation.

When foreign materials are introduced into



Laboratory apparatus used at the National Bureau of Standards for simultaneous measurement of flame speed and flame temperature. The pressure, temperature, and composition of the gaseous mixtures that are burned are carefully controlled and accurately measured. The large lamp at the right is a calibrated tungsten filament lamp used for determining the temperature of the flame.

flames the color of the flame is often characteristic of the added material in accordance with the previously mentioned principle of Kirchhoff. For example, a small trace of sodium will turn a flame a vivid yellow. The streaks of yellow frequently seen in the flame of a gas range are caused by table salt (sodium chloride) on the burner. The property of materials to emit characteristic radiation in a flame is used as a convenient and precise method for both qualitative and quantitative analysis in many industrial, chemical, and biological laboratories. Concentrations of sodium as low as 1 part in 100 million are detectable by this method of flame photometry.

A more detailed examination of the radiation from flames is made possible by spectroscopes that separate the colors sufficiently to enable observation of fine detail. In addition, the use of detectors such as photographic plates and photoelectric cells permits the range of observation to be extended beyond the visible to the infrared and ultraviolet. By such studies it has been possible to observe both the final products of combustion, such as water vapor and carbon dioxide, as well as intermediate molecules or free radicals, such as OH, CH, and C_2, many of which have no name other than the chemical symbol. These intermediate species are chemical compounds that are not ordinarily found because they react quickly with one another and with other molecules to form more stable structures. No adequate theory exists of the step-by-step processes whereby the original reactants are transformed into the spectroscopically observable intermediates and then into the final products. The entire reaction takes place so quickly (in less than 1 μsec) that it is difficult to sort out and comprehend the individual steps.

A substantial amount of current research on radiation from flames is based on the study of energy distributions in hot gases. In general the total energy of a gas molecule is made up of translational, rotational, vibrational, and electronic energy. The translational energy is caused by the motion of the molecule relative to a fixed reference system. In a gas it is this energy that is observed by ordinary temperature and pressure indicators. The vibrational energy is caused by the motions of the atoms of the molecule relative to each other; the rotational energy is caused by the spinning of the molecule about its center of mass; and the electronic energy is caused by the positions of the negatively charged electrons relative to the positively charged nuclei.

It is known from quantum theory that the energy of a molecule does not change continuously but jumps suddenly from one value to another. To accomplish this the molecule must acquire or get rid of discrete bits of energy called quanta. These quanta in many cases take the form of radiation. Since the molecules can exist only at definite energies, these energies can change only by definite amounts, giving rise to characteristic radiation from each type of molecule in agreement with Kirchhoff's principle. Thus from the observed infrared, visible, and ultraviolet radiation it is possible to make certain deductions about the electronic, vibrational, rotational, and translational energies of the molecules emitting the radiation.

In general the temperature of a substance is a measure of the energy of the molecules of that substance; the higher the temperature, the higher the energy. Whenever a gas is in a state of thermal equilibrium—that is, when the amounts of various forms of energy averaged over a large number of molecules are not changing with time—the temperature is defined by the energies. Stated differently, at a fixed temperature the proportion of energy in any of the various forms is known in detail from statistical mechanics and quantum mechanics. However, any sudden change in the temperature, composition, or pressure of a gas will produce a condition of nonequilibrium, and some time is necessary before equilibrium is again attained. In the reaction zone of a flame, a state of equilibrium does not exist while the reaction is going on, because the reactants and products are constantly changing and the average temperature of the gases is increasing.

With the aid of quantum mechanics, the measured distribution of radiation intensity can be used to obtain the temperature when the gas is in equilibrium. However, in nonequilibrium cases, a temperature in its usual meaning does not exist.

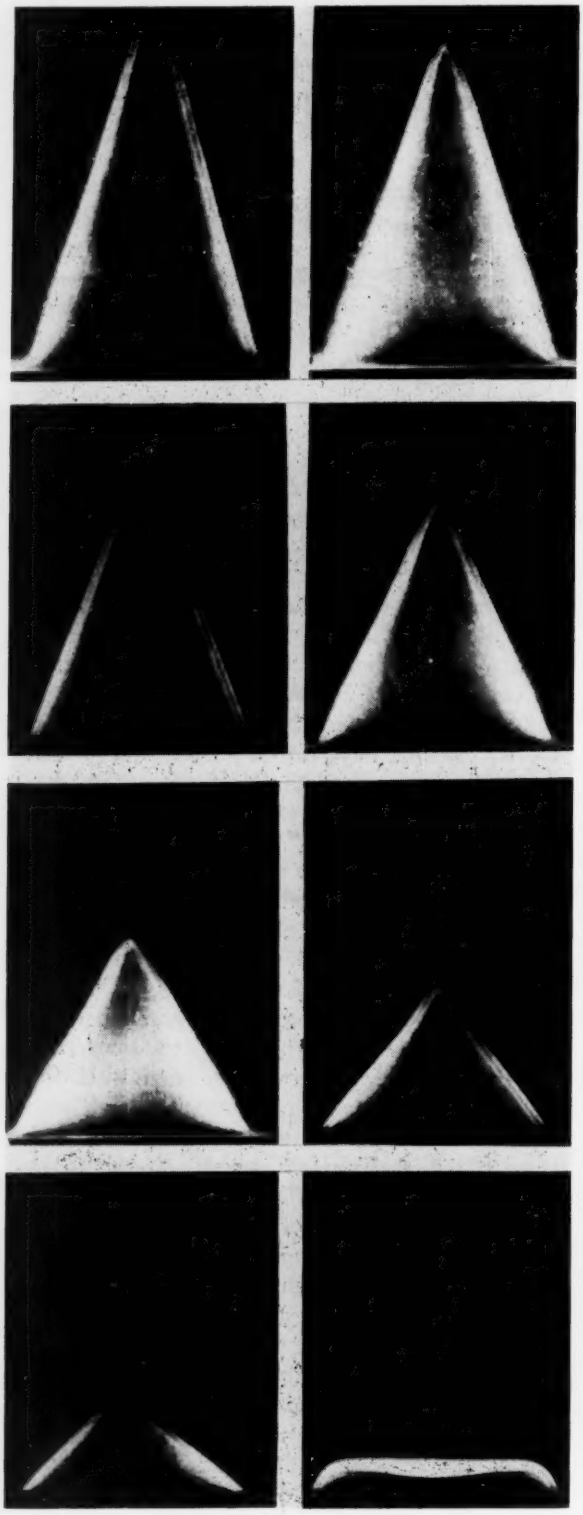
Since ordinary liquid-in-glass thermometers are not usable above 500°C and since most of the high-temperature thermocouples are not used much above 1500°C, some radiation means of measuring temperature is needed for gases burning at higher temperatures. In nonequilibrium regions of flames the intensities of radiation often indicate an apparent equilibrium distribution that leads to a measurement of so-called "rotational or vibrational temperatures." From studies of such "abnormal temperatures" it is hoped that the time required to reach equilibrium can be measured and that some new ideas can be uncovered concerning mechanisms for energy exchange. It is also hoped that "rotational and vibrational temperatures" can be related to quantities that are useful in the practical application of flames.

Flame of propane and oxygen burning at a temperature near 3000°K (2727°C). The bright cone near the burner is the reaction zone. In the luminous region beyond the reaction, the velocity of the particles is more than 3500 cm/sec.

Measurements of temperatures of flames or hot gases are exceedingly difficult even with radiation techniques. Not only are these problems connected with the nonequilibrium conditions in some regions of the flame, but there are also large temperature gradients from the very hot region to the relatively cold surrounding atmosphere. Since measurements of radiation generally include contributions from all regions of the flame in front of the detecting equipment, the observed intensity of the radiation is an average value from several temperature zones. Reasonably trustworthy radiation methods of measuring the temperatures of flames have been developed but the absolute accuracy of such measurements is probably not very good.

The speed with which a flame propagates into an unburned gas mixture depends upon composition, temperature, amount of turbulence, pressure, proximity to surfaces, viscosity, diffusion rates, and perhaps upon other factors. In order to simplify the observation of flame speed, efforts are being made to measure a quantity called the "burning velocity," which depends only upon the gas mixture, the pressure, and the temperature and is independent of the apparatus in which it is measured. "Burning velocity" is defined as the speed of travel of the flame front into a combustible mixture in a direction normal to the flame front, when the burning is unaffected by surfaces. The numerical value of the burning velocity is much lower than the speed of sound, ranging from the order of tens of centimeters per second for hydrocarbons mixed with air to around 1000 cm/sec for hydrogen mixed with oxygen. A combustible mixture may also support a detonation that travels many times the speed of sound attaining values between 10,000 and 350,000 cm/sec.

The tremendous difference between normal burning and detonation velocities results from the difference in the methods by which the flame is propagated. Under conditions of normal burning the flame travel is limited by heat conduction and the diffusion of particles that easily react with the unburned gas. In contrast, detonation flames are propagated by a self-sustaining shock wave traveling at velocities limited only by the chemical nature of the mixture and little influenced by the pressure and temperature of the gases. Although there are some similarities between detonation in tubes and "knock" in automobile engines—both travel at very high velocities—they are not exactly the same phenomena. For example, these differences are demonstrated by the fact that "anti-



Flames of premixed methane and air, showing the effect of varying inlet flow rates. The outer image in each photograph is a picture of the flame's own light. The inner image is a schlieren shadow picture. An estimate of the flame thickness is obtained from the distance between these images.

knock" agents, such as tetraethyl lead, which have a large effect on preventing "knock," have very little effect on detonation in tubes. Although, in general, it is known from experiment under what range of physical and chemical conditions ordinary burning and detonation are possible, no theory has yet been devised that can predict reliably what will happen under a new set of circumstances.

From the time of Dalton early in the 18th century, it has been known that chemical reactions take place in such a way that the reactants and products occur in definite proportions. For example, 2 vol of hydrogen when mixed with 1 vol of oxygen form 2 vol of water vapor, as is symbolically represented by the equation

$$2H_2 + O_2 \rightarrow 2H_2O.$$

This simple statement of the reaction leaves out all consideration of how long it takes the reaction to occur or under what conditions it is possible. It is also known that this reaction does not take place by direct combination of hydrogen and oxygen molecules but, instead, proceeds by a complex chain of reactions involving such structures as O, H, OH, HO_2, and H_2O_2.

In a flame in which hydrogen and oxygen are burning it is quite likely that not all the hydrogen and oxygen are used. As a result, the burned gases will contain hydrogen and oxygen molecules as well as water. In addition, at the high temperature of the hydrogen-oxygen flame there is some dissociation of the molecules, and atoms of hydrogen and oxygen, as well as molecules of OH, will also be present. The presence of atoms and molecules other than water has the very important effect of decreasing the amount of heat available from the reaction.

When more complex burning mixtures are used, the number and kinds of final products are greatly increased. In certain cases it is possible to make theoretical calculations of final temperatures and products. These calculations are based upon the thermodynamic properties of gases and make it possible to estimate the amount of heat available from burning. Experimental techniques for measuring the temperature and the concentration of the products have not reached sufficient precision to enable an accurate test of the validity of the theory when applied to flames.

The foregoing discussion has been concerned mainly with the simplest type of flame, the burning of premixed combustible gases. This is probably the least important type of burning from an industrial or transportation point of view. However, the more practical forms of combustion present a host of complicating factors. The emphasis on

simple flames has the positive aspect of increasing our knowledge in definite, though small, steps.

The development of land, sea, and air transportation, industrial processes, and power generation has necessitated the utilization of complex types of burning using partially mixed gases as well as solid and liquid fuels. Much information is available concerning such flames, but the information is largely a catalog of the results of a vast number of experiments. While such inductive knowledge is extremely valuable, it suffers from the weakness of being uncertain in applications to new and untried cases. Thus developments in basic theory would help in the all-important task of using flames under optimum conditions of safety and economy.

It is quite likely that it will be a long time before an adequate over-all theory is formulated that will both account for many observations concerning the propagation of a flame into a flammable mixture and also predict the results of new and untried experiments. However, a tremendous amount of information exists that is being translated daily into practical uses. A large range of useful temperatures is available. The flame from an ordinary gas stove may burn as high as 1800°C, while 3000°C is attained with an oxygen-acetylene welding torch. These are not the extreme temperatures of flames but represent average cases that lie between the low value of 350°C found in certain "cool flames" of hydrocarbons and the high of more than 4000°C that is possible with mixtures of hydrogen and fluorine. Recent experiments have shown that metals such as beryllium and aluminum combine with oxygen at temperatures exceeding 5000°C. Sufficient information is available about the flame speeds of many types of mixtures to allow for the design of apparatus and machinery that will operate within a desired range of utility and safety.

The study of flames still offers serious problems to research workers in physics, chemistry, and the applied sciences. In addition to the many unsolved problems of flames *per se,* the study of combustion provides a method of approach to many problems concerning molecular energy, energy transfer, reaction rates, and the spectra of excited molecules.

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A Flame

This month's cover is a photograph of a flame of premixed methane and air burning on a ½-in. nozzle at a temperature near 2200°K. Burning takes place just inside the surface of the bright inner cone. The outer envelope consists of hot gases, the products of combustion. This photograph was made at the National Bureau of Standards in connection with research directed toward establishment of temperature standards for burning gases.

Other studies at the NBS also seek to determine what substances undergo reactions in flames, where the combustion takes place, what effect the rate of flow of the fuel gas has on the flame and, finally, how these factors are related to flame size, color, temperature, and speed of propagation. It is hoped that the information thus obtained on the fundamentals of combustion may lead eventually to greater efficiency in ramjets and gas turbines and in industrial processes based on flames.

Acceptance of Science

ALAN T. WATERMAN

Dr. Waterman, who was trained as a physicist at Princeton University, is director of the National Science Foundation. Prior to 1951 he served with the Office of Scientific Research and Development during World War II and as chief scientist of the Office of Naval Research. From the close of World War I until 1948, he was a member of the faculty of Yale University. He also had visiting professorships at King's College, London, and Massachusetts Institute of Technology. He is a coauthor of "Scientific research and national security," which appeared in the April 1954 issue.

IT is a temptation to think of research, as we know it today, as a phenomenon of our present age. The vast modern laboratories and the expensive and complex equipment that fills them bear so little resemblance to the crude apparatus of our scientific forefathers that they seem more nearly the product of the machine age than the result of a long evolution.

According to the OED, the word *research* first appeared in the language in the 17th century, a date that coincides with the beginnings of modern science. The root word *search*, however, meanders back through middle English, old French, and Italian, through the Latin *circare*—"to go round, go about, explore"—until it finally comes to rest in the ancient word for *circle*. I find this derivation esthetically very satisfying, as though it were symbolic of the unceasing efforts of scholars to see wholeness and order in the vast confusion of nature. Three characteristic activities of *Homo sapiens*—invention, discovery, and reflection—go back a very long way and might be properly classified as research, I believe, if we think of it in the broad sense of "to search for."

These early forms of "research" are closely linked to such elemental motivations as subsistence and survival, the satisfaction of natural curiosity, the urge to find creative expression. The means of satisfying these basic needs and desires change with the passing centuries, but the motivations do not. Thus as he approaches the finite limits of the natural resources, man busily seeks to discover or to invent new ways of meeting his essential needs of food, fuel, and fiber.

The curiosity that was once directed primarily toward the mysterious phenomena of eclipses, earthquakes and storms, and the cycles of seasons now peers into the interstellar spaces and into the subatomic world of the nucleus. As one problem is solved, the mind of man restlessly ranges the universe, looking for new worlds to conquer.

The urge to create that has satisfied itself in art, in poetry, in music, and in literature seeks in science to describe, through the formulation of laws and theories, the marvelous order that man's patient observation has discerned in nature. Paul Sears (*1*) expressed it so well when he said:

> We have fruitfully pursued knowledge to the smallest knowable constituents of matter, even to the mysterious bridge between matter and energy. At the other extreme we have studied the most complex manifestations of matter and energy in the living community. Our challenge now is to a new and grand synthesis, a new unity between these extremes of knowledge.

As man has evolved and his gropings after knowledge have gradually assumed the form and shape of modern research, public attitudes toward the discoveries of science have followed a curious pattern. One of the most fascinating chapters in the history of human thought is the way in which the enunciation of new theories and discoveries has been characterized first by rejection and hostility, then by a reluctant acceptance, and finally by indifference—as something new takes their place.

I have previously remarked, as a rough generalization, that a highly significant discovery is confirmed, attested, and accepted by leaders in its special field perhaps within a decade; it becomes understood and accepted by the general body of scientists in that field within the generation; and some time during that century it will be generally accepted by literate mankind.

I should like to address myself to this matter of the acceptance of science (*2*), for it is a matter that concerns society very deeply—and never more so than in the present day when some of the findings of science have such new and terrible implica-

tions. The gap between rejection and acceptance has often been fraught with peril for the individual scientist. The scholar who enunciated a new theory to explain some natural phenomenon might find himself being roasted figuratively or possibly even literally, if the theory ran counter to some ingrained popular belief. In our present-day sophistication we like to view these episodes as historical aberrations. Do we not, for example, feel ourselves far removed from the oppression of Galileo, the burning of Bruno, the beheading of Lavoisier?

I am not so sure, however, that the events of recent decades sustain us in this complacency. Invention and discovery have generally encountered three types of resistance: economic, religious, and intellectual. The history of invention is filled with examples of new devices that have been fought because of their potentialities for producing technologic unemployment. Many great scientific discoveries have seemed, initially at least, to be incompatible with religious doctrine. And finally, a significant portion of scientific knowledge has had to wait a long time for acceptance, because it could not be understood by the public or because it seemed to contradict common sense.

If we concede that at least there have been no burnings or beheadings, the record of the present century does not depart from this pattern in any significant way. I should like to illustrate my point in the lives of three of the men who have most greatly influenced the intellectual life of today: Darwin in the biological sciences, Einstein in the physical sciences, and Freud in the medical sciences. Each of these, in his own field, wrought a revolution in traditional thinking as bitterly fought as any of the great intellectual revolutions of the past.

Darwin's magnificent theory of the origin of species and the descent of man had fully as much impact on his own time as the Copernican theory had on the 17th century. Not only did his new theories run head-on into the religious doctrine of special creation, but they encountered the peculiar opposition that Walter E. Houghton has called "Victorian anti-intellectualism." Houghton (*3*) describes the plight of the independent mind of the day as follows:

> Even if the thinker himself believed that the service of truth was worth any actual, or supposed, ill consequences that might result, he was often deterred by the fear of social stigma and its potential threat to his public career. Under such circumstances the better part of wisdom, for anyone not prepared for martyrdom, was to narrow his "thoughts and interest to things which can be spoken of without venturing within the region of principles, that is, to small practical matters" (*4*). Was there ever another age where the potential cost of speculation ran so high? At one and the same time, the thinker was threatened both by the fear of what he might do to society and the fear of what society might do to him. . . .

If the Darwinian theories met with both religious and intellectual opposition, the Einstein theory of relativity is surely one of the least-understood theories in the history of science. Even among scientists, its initial failure to be accepted was characterized by a lack of understanding. Although the special theory of relativity was first formulated in 1906, news of it reached the public only in 1919, as Hans Reichenbach (*5*) has pointed out, when an English expedition sent to observe an eclipse of the sun reported the first astronomical confirmation of Einstein's predictions.

It is of some interest today to note contemporary editorial comment. Thus the *New York Times* (*6*), although it reported factually in its news columns that Sir Joseph Thompson, president of the Royal Society, had described the Einstein theory as "one of the greatest—perhaps the greatest—of achievements in the history of human thought," commented skeptically in its editorial columns a week later (*7*):

> A sense of proportion ought to be useful to mathematicians and physicists, but it is to be feared that British astronomers have regarded their own field as of somewhat greater consequence than it really is.

In the light of the application of Einstein's now famous equation regarding the equivalence of mass and energy, the following sentence from the *London Times* of 28 November 1919, also falls strangely on our ears: "It is agreed that the new conception will make little difference to the practical world." Still, the *Times* of London comes off rather better than its New York counterpart, for it goes on to say:

> . . . the change in philosophy and metaphysics is fundamental. With all respect to the ripe wisdom of our esteemed correspondent, Professor Case, we cannot accept his view that there are still left to us the conceptions of absolute space containing all relative spaces and absolute time containing all relative times.

Last year, when Einstein announced his unified field theory, the *New York Times* commented in a chastened tone (*8*):

> If he proves to be right, his unified field theory will be a supreme achievement of the human intellect. There is much more than mathematical genius in all of this. The equations which were published yesterday, and which must have been as undecipherable to the public as the hieroglyphics of an Egyptian tomb, must be regarded as a kind of sublime mathematical poetry—an expression of a deep, almost mystical conviction that if we can only disentangle our confused sense impressions we shall come a little nearer to what we call "reality."

Does this mean that the public is more enlightened now than it was 35 years ago when the world had the first impact of Einstein's thinking? It would be encouraging to think so. But we must remember that these same decades have witnessed with what awesome effectiveness theoretical science can be applied.

Freud, too, may be regarded as the perpetrator of a revolution, which, in the words of the *Saturday Review* (*9*) "passed far beyond the field of medicine, in which it began, to every form of intellectual activity." We are still in the midst of this revolution, still witness to the innumerable controversies that rage among the various schools of thought. It is therefore more difficult to assess its impact than to evaluate those that have already won the acceptance of mankind. But it is also of considerable interest to observe the process as it goes on. Freud was a pioneer who ventured far indeed from the traditional shores of science. In attempting to apply the methods of science to the workings of the mind, and particularly of the disturbed mind, he ventured into a field where there were more unknowns than knowns. E. Pumpian-Mindlin has placed the matter in helpful perspective (*10*):

> Freud entered the scientific scene at a time when the impact of the Darwinian evolutionary theory was tremendous. The bewildering chaos of animal life had suddenly become ordered, logical, coherent, rational. The principle of evolution had revolutionized the approach to life. Man had taken his place in the evolutionary scale as a highly developed animal. He was no longer a divine creation, static and unchanging, but a phenomenon of nature, who could be studied and examined as a product of natural forces. If biologically man was a natural phenomenon, then perhaps psychologically he was also. In this intensely biological and rational atmosphere Freud approached the problem of the neuroses of man.

In his own lifetime, Freud encountered much of the hostility and resistance with which so many scientific discoveries have been received. A recent popular article points out that (*11*):

> . . . Freud received practically no honors of any kind until 1930, when he was already 75 and when he was awarded the Goethe prize for literature. He was not elected to honorary membership in the American Psychiatric Association until 1936, when he was 80. And he was voted into Britain's august Royal Society only some weeks before he died of cancer, September 23, 1939.

Freud himself was fully aware of the public attitude toward the new field, for he comments dryly in the opening lecture of *A General Introduction to Psychoanalysis*:

> . . . if any one of you should feel dissatisfied with a merely cursory acquaintance with psychoanalysis and should wish to form a permanent connection with it, I shall not merely discourage him, but I shall actually warn him against it. For as things are at the present time, not only would the choice of such a career put an end to all chances of academic success, but, upon taking up work as a practitioner, such a man would find himself in a community which misunderstood his aims and intentions, regarded him with suspicion and hostility, and let loose upon him all the latent evil impulses harboured within it.

Nevertheless, the influence of Freud upon our own time has been immense. Probably no scientific theory has more profoundly influenced the arts, and its effects upon many of the social sciences have been far-reaching and pervasive. One of the striking examples of its influence on our culture has been the extent to which the Freudian vocabulary has become a part of everyday speech. The final assessment of Freud's work has perhaps not yet been made, but in the meantime we have the opportunity to observe a great deal of interesting research in a field that is still being pioneered. Only last summer (*12*), for example, came word from the 14th International Congress on Psychology in Montreal of work that may confirm experimentally some of the theories of Freud. A noted neurosurgeon reported that a patient, whose brain during an operation was stimulated by electric impulse, relived long-forgotten experiences, suggesting to the surgeon the existence of a nerve center that continuously records a stream of consciousness.

The "case histories" of Darwin, Einstein, and Freud are not the only ones, of course, that prove that the old pattern of rejection and acceptance persists into our own time. We could draw upon others; we could go further and demonstrate that we are still judging men on their ideas as well as their actions. But the important thing is to ask whether attitudes toward science and scientists

may not have more significance in an atomic age than they did when less was at stake.

It is a sobering thing when a man of the stature of E. D. Adrian tells an audience of his peers (*13*):

> We must face the possibility that repeated atomic explosions will lead to a degree of general radioactivity which no one can tolerate or escape.

Where should we turn for help amid the dangers of our age except back to science?

It was Congressman Hinshaw who reminded us recently (*14*):

> Atomic development holds great promise for the peacetime side of our lives. The same men and women who can give us military strength can also give us fuller, better lives in the decades ahead. Inspire these people; seek them out. Make them feel the honor and responsibility which is properly theirs. And they will help us all achieve the things which today are no more than promises, things which can be realities tomorrow.

We must recognize, however, that if as citizens we make extraordinary demands upon scientists for our defense and welfare—as well as for our cultural enrichment—we have an obligation to permit the existence of an intellectual climate conducive to creative activity. In many respects the intellectual climate of today is an improvement upon that of previous eras. In most of the world illiteracy is the exception rather than the rule, and learning, no longer the monopoly of a privileged few, does not enjoy the blind respect and authority that characterized it in other ages. Education and democracy have produced not only a greater understanding on the part of the public but a healthy skepticism as well, and science must explain not only "the what" but also "the why" and "the how." Modern communications have made possible a freer exchange of information among scientists and also between scientists and the public. Science news finds its way into the headlines along with news of a more overt nature.

Nevertheless, many aspects of the intellectual climate of today leave much to be desired. The increasing premium placed on conformity in all fields makes us wonder whether we may not be drifting toward the same kind of anti-intellectualism that characterized the Victorian age. In science there are disquieting signs that there is a great deal, particularly about the nature of research, that the public does not understand. For example, the Manufacturing Chemists Association was greatly surprised to learn, as the result of a recent survey (*15*), not only that the chemical industry is widely misunderstood but that a considerable portion of the population actually feels hostile toward the industry!

To us in the National Science Foundation it has been clear from the very beginning that the nature of basic research, as contrasted with applied or industrial research, is far from being understood by the American people. At a critical juncture in our history, when much in our future may depend upon the soundness and originality of our basic research, the tendency has been to hold its support to an absolute minimum. It is also disquieting to observe that a substantial body of public opinion blames the scientists for the dilemma brought about by the creation of weapons for mass destruction.

Questions for our own time are these: Does society on the whole accept, reject, or tolerate scientific research? Is research, particularly basic research, an intellectual luxury or a necessity? Must this question be answered differently in different fields of research? Does the latent hostility toward science on the part of society mean that it may attempt to limit scientific research activities in general or by specific fields?

Implicit in all these questions is the danger that society may be moving either toward a rejection of science or toward an active control of its direction. The example of Germany in World War II demonstrated that the attempt to control the direction of science can be disastrous. We know that the same sort of thing is going on in the Soviet Union, but we are not in position to assess the results fully.

We do know, however, that in the Western World science and other branches of learning have grown and flourished in an atmosphere of freedom. Science, in particular, can move ahead only if scientists are free to think as unconventionally as they please and to pursue those unexpected leads that are constantly developing as part of the scientific process.

But the scientists, too, have a heavy responsibility in the matter. It devolves upon them to make clear to the public, not only their general aims and objectives, but especially the methods by which these are achieved. The implications of science are now so great for all of us that scientists can no longer afford the special luxury of communicating only with their fellows. The results of science exert a vast influence upon the political and economic questions of our day. We can see that we are on the threshhold of a new "industrial revolution" vaster and more far-reaching in its implications than the first. The potentialities that have opened up in one field of the physical sci-

ences will almost certainly develop in other broad areas of the natural sciences. With regard to the social sciences, there are already strong indications that research that relates to society and to the individual will be increasingly subject to critical scrutiny by the general public. I see no good way of dealing with the situation other than by frankness and open discussion. It is therefore highly necessary that the public in general, and our public officers in particular, have sufficient understanding of the issues involved to make wise decisions concerning them.

Thus far I have stressed the purely materialistic reasons why science must remain free. The nature of the scientific advances of the past decade, coupled with the political idiosyncracies of our times, makes the understanding and advancement of science a practical matter of survival. There is a deeper and more fundamental issue involved, however; for I doubt that many of us would wish to survive in a purely physical sense if in the process we should lose our cherished intellectual freedom. For as Paul Sears has so eloquently put it (*16*):

> It is the great destiny of human science, not to ease man's labors or prolong his life, noble as those ends may be, nor to serve the ends of power, but to enable man to walk upright, without fear, in a world which he at length will understand and which is his home.

Science can achieve this destiny only if its path and all the other paths by which men seek the truth remain free and clear. Society has the right to demand of scientists the scrupulous fulfillment of their duties as citizens, the same undeviating loyalty that it exacts of other citizens; but beyond that, we shall be in trouble when a man hesitates to express, or even to formulate, a bold new idea because of the possible adverse reaction of public opinion.

Emerson (*17*) reminds us that "all history teaches wise men to put trust in ideas, and not in circumstances." As we build our defenses against the threat of possible aggression, therefore, let us leave the way open and clear for the healthy growth of new ideas; for in the long run a nation is free only if its citizens are free to think and to give voice to their thoughts.

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Science, by itself, provides no panacea for individual, social and economic ills. It can be effective in the national welfare only as a member of a team, whether the conditions be peace or war. But without scientific progress, no amount of achievement in other directions can insure our health, prosperity and security as a nation in the modern world.—VANNEVAR BUSH.

Some Problems in Large-Scale Culture of Algae

HAROLD W. MILNER

Following his training at Colorado College, Mr. Milner spent 2 years as an analytic chemist. Since 1928, he has been in the Department of Plant Biology of the Carnegie Institution of Washington in Stanford, California, where he has worked on problems in plant biochemistry, including plant pigments, the formation and dissolution of starch in leaves, the photochemical activity of isolated chloroplasts, the chemical composition of algae as affected by environment, and then the large-scale culture of algae. This article is based on a paper given at the symposium on Uses of Large-Scale Algal Cultures, *AAAS meeting, Boston, Massachusetts, 29 December 1953.*

IN most of the thinking about the large-scale culture of algae, the emphasis has been on the possibility of using algae for food. The world's food problem is only one part of the larger problem of how man can make more efficient use of the energy the earth receives from the sun. Almost all the energy available for use by man represents solar energy that has been converted into chemical energy by the activity of plants. The few exceptions to this include water power, wind power, volcanic heat, and nuclear reactions. Plants that grew in the remote past have been converted by geologic processes into coal and oil, our present great reserve supplies of energy. When these reserves are gone, it appears that man will have to depend on the day-to-day supply of sunshine to satisfy his ever-increasing demands for energy.

Every day the earth receives far more energy from the sun than man uses in all his activities. The only part of this energy that we know is converted into chemical energy and stored in the light used in plants. Man has not yet devised efficient ways to use the low-level energy of sunlight directly to drive machinery or to heat furnaces. He may do so in the future, but even if he does he will still be entirely dependent on plants for one vital source of energy, his daily food.

Farming is an extremely inefficient way of converting light energy into food. Most farm crops use about one-half of 1 percent of the light that falls on the farm. The best ones use hardly more than 2 percent. Only the green parts of the plant work at making food. Part of this food is converted by the plant into stems and other structural elements that are waste products as far as feeding man is concerned. After the crops are grown, half or more of the total dry weight of the plants is made up of indigestible material. As a result only about one-quarter of 1 percent of the available sunlight is converted into food energy that man can use. One way to increase man's food supply would be to obtain more efficient conversion of solar energy by plants.

An ideal food plant would cover all the growing area all the time. No light would be wasted because it fell on bare ground. This plant would convert much more than a fraction of 1 percent of the light energy into energy stored as food. It would have more protein and fat and less indigestible material than the kinds of plants we use now.

It does not seem reasonable to expect that vegetables, grains, or root crops can be made to do what we want our ideal plant to do. Maybe we should look for some other kind of plant. There is more vegetation growing in the waters of the earth than there is on the land. Many of the water plants are simpler in structure than the higher plants. In one of these, the unicellular green alga, all the functions of the plant are carried on inside a single cell, microscopic in size. There is a maximum of working plant tissue and a minimum of structural material. The one-celled green algae fit the list of things we want in our ideal food plant in several ways: (i) a culture of algae can be maintained continuously and it will therefore absorb all the light that falls on it; (ii) algae can convert much more of the light they receive into stored energy than farm crops can; (iii) chemical analysis shows a much higher percentage of protein and fat in the green algae than is found in any of our customary food plants when the *entire* plant is con-

sidered; and (iv) algae have less waste material.

The algae have another great advantage not found in the kinds of plants grown on farms. The conditions for growing algae can be controlled and varied, and the effect on the algae can be studied. Most of the experimental work on the growth of algae has been done with a green unicellular alga called *Chlorella,* which has long been for the plant biologist what the guinea pig has been for the animal biologist. Hundreds of workers have contributed to our knowledge about the growth of algae.

Spoehr and Milner found that the proportion of carbohydrate, protein, and fat in *Chlorella* can be changed by varying the conditions of growth. It is easy to grow *Chlorella* with very great differences in composition or to reproduce any one composition. The carbohydrate content can be varied from 6 to 38 percent of the dry weight. Protein can be at any desired level between 7 and 88 percent. The most striking variation is in the fat content, which can be obtained at any value between 5 and 75 percent. It is probable that other green algae could be changed in composition at will.

Chlorella is the fastest growing of the green algae that have been studied. Under conditions favoring the most rapid growth, its composition is about half protein and 7 percent true fat on a dry weight basis. The combination of rapid growth and controllable composition has aroused interest in the possibility of growing green algae on a commercial scale. If it could be grown in large quantities with the same yield as in laboratory cultures, 40 tons of dry *Chlorella* per acre, per year, could be produced. This would be equal to 20 tons of protein and 3 tons of fat per acre, per year, many times the present yields of these needed foods. If algae are to contribute to the world food supply we need to know first how great a yield can be obtained from very large cultures.

Several research groups have made extensive studies of the biological factors affecting the growth of *Chlorella* under conditions approaching those to be met in large-scale cultivation. One of these groups is led by Tamiya at the University of Tokyo, another by Jack Myers at the University of Texas, a third by M. J. Geoghegan at Jealott's Hill in England.

A demonstration that *Chlorella* can be grown continuously in direct sunlight was made by Paul Cook at the Stanford Research Institute. He grew *Chlorella* in a 4-in. glass tube 6 ft long, fitted with a cooling device. A photoelectric control regulated the removal of culture and the addition of fresh medium to maintain the culture at any desired density. The yield of *Chlorella* was less than expected.

What is the limit to the quantity of algae that can be produced? We cannot expect more algae than can be gotten by complete conversion of the energy of sunlight into plant material. In fact, we cannot use more than half the energy in sunshine by growing algae. Plants can use only the energy in visible light. The other half of the energy from the sun is in unusable infrared rays and heat. A hypothetical figure for the maximum yield of plant material can be obtained by assuming complete conversion of the energy in visible light by the plant. We know that we can never exceed this figure, but we also are reasonably sure that we can never reach it.

Plants do not use light with 100-percent efficiency. The best experimental demonstration of the conversion of light energy to plant material showed an efficiency of about 25 percent. This experiment was carried on under highly specialized laboratory conditions. To use 25 percent for the possible energy conversion of the visible part of sunlight to plant material gives a purely theoretical maximum figure, but it is the best one we have. If a commercial installation for algal culture could work as efficiently as the best laboratory experiment, our theoretical yield of algae would represent the conversion of one-eighth of the total energy of sunshine into plant material.

The use of plants as converters of solar energy leaves much to be desired. An engineer expects to get more than 12.5-percent efficiency in the transformation of one form of energy to another. Nevertheless, the plant is still the only converter that will give us energy in one form we must have, food.

The amount of light energy received from the sun in a year is very different for different locations on the earth. At San Jose, California, the year-around average energy received in the spectral range between 4000 and 7000 A is 170 calories per square centimeter per day (170 cal/cm^2 day). If 25 percent of this energy were converted to algal plant material having a heat of combustion of 5.77 kilocalories per gram (5.77 kcal/g), it would be theoretically possible to obtain 120 tons of dry algae from an acre in a year. According to this, our estimate of 40 tons per acre, per year, would suppose 8-percent efficiency in light utilization. This is an optimistic, but perhaps not impossible, figure to reach.

Wassink, Kok, and van Oorschot in Holland measured the efficiency in use of light by *Chlorella.* Kok measured the amount of light energy supplied

to a laboratory culture, weighed the algae produced, and determined their heat of combustion. His figures are the basis for the theoretical yield mentioned in the foregoing paragraph. For growing *Chlorella* outdoors, the Dutch workers used tanks 1 meter square with 300 liters (300 lit) of culture per tank. Yields of 2.7 to 7.2 grams dry weight per square meter, per day (2.7 to 7.2 g (dry wt.)/m^2 day), were found, with calculated efficiencies of 2 to 3 percent for use of light. The same authors compute the efficiency of various crop plants to be from 0.45 to 2.2 percent. Calculated from the stated yields and efficiencies, the theoretical yield for 25-percent use of light would be 80 tons per acre, per year. This is not in disagreement with the 120 tons computed for San Jose when it is taken into account that the location in Holland is about 15 deg. farther north than the one in California.

Going in the other direction, the greater amount of solar energy received in the tropics would make possible a theoretical yield of 150 tons per acre, per year. Large-scale culture of algae under the simplest conditions was carried on by Jorgensen and Convit at Maiquetia, Venezuela. They grew a mixture of several kinds of algae in a solution of commercial fertilizer in 200 open, unglazed clay bowls. Each bowl was 40 cm in diameter and held 15 lit of culture. Evaporation of water from the porous bowls kept culture temperature at about 26°C, even under the tropical sun. Every day the cultures were stirred, and water was added to make up for evaporation. Harvesting by settling and decantation gave a "soup" containing about 10 percent by volume of algae. The workers estimate an annual yield of 204 kg of organisms from the 200 bowls. This yield is equivalent to 36 tons per acre, per year. It is possible that the great amount of light in the tropics, as well as the fact that the cultures were probably never too cool to grow actively, explains the large yield from this very simple method of culture.

It is clear that efficient use of light is of paramount importance for good yields of algae from large-scale cultures. Artificial light would be too costly. At theoretical 25-percent efficiency in use of light, it would take 80 kw hr of electric power to grow a pound of algae. Therefore we must depend on sunlight. All the energy in sunshine, except for the fraction used by the algae for growth, will appear as heat in the culture. This makes cooling necessary, for the kind of algae in which we are interested will not live in hot water.

Now we come to a paradoxical situation. Sunlight is a too-low-level form of energy to run a heat engine, but it is far too intense to be used directly by algae. Nature has fitted the algae to use dim light. If just enough light is available to make growth balance respiration, the algae neither gain nor lose weight. This is called the compensation point. In the low range of light intensities, algae can convert nearly 25 percent of the light energy into chemical energy stored as plant material. The amount of growth is proportional to the amount of light. At medium intensity of light, the growth of algae reaches its highest value, but the efficiency of energy conversion decreases. At around 500 to 1000 foot-candles (500 to 1000 ft-ca), what is known as light saturation occurs. More light than this will not increase algal growth; hence, the efficiency in use of light goes down as the light becomes brighter than saturating. At the very high intensity of sunlight, algae can use only a small fraction of the energy. Moreover, very much exposure to full sunlight will damage or kill the algae. If we wish to grow algae efficiently in sunlight, we must have some way of reducing the intensity without wasting light.

An interesting plan for efficient use of sunlight is proposed by Evanari and Mayer at Hebrew University in Jerusalem. They describe an optical system to "dilute" the sunlight below saturating intensity and to distribute the light at low intensity all the way from top to bottom of a meter-deep culture.

Another way to dilute the light is to let part of the algae in a culture shade the rest. Of course, the culture will have to be stirred to prevent part of the algae from remaining too long in full sunlight. Let us consider what happens in a culture with a fixed area exposed to light as the amount of algae increases. We shall assume that all other growing conditions are favorable and consider only the effect of light. At first the growth of algae is logarithmic, proportional to the number of cells. Here the growth rate is greatest, but the yield is small because the number of cells present is small. This condition lasts only while the culture has so few cells that each one is continuously exposed to light of saturating intensity, and some light is wasted. After the number of cells increases until their growth is limited by the amount of light they receive, the yield becomes highest and continues the same each day for a time, while the growth rate is decreasing because the number of cells is becoming larger. All the light is being absorbed. Later there will be so many cells in the culture that part of them will not get enough light to grow.

Then both growth rate and yield decline until they reach zero. The static state is reached when the culture becomes so dense that the growth of the small part of the cells getting light is balanced by the respiration of the large part of the cells in the dark. The culture as a whole has reached the compensation point.

By continuously harvesting algae and replacing medium, the culture can be kept in a steady state at any desired cell density. The maximum yield will be obtained over a range of densities. More cells will be harvested per unit volume of culture if the density is the highest that is consistent with maximum yield.

The amount of light absorbed by a culture depends on the number of cells per unit of illuminated area. The volume in which these cells are distributed is determined by the depth of the culture. The proper relationship between the depth and cell density of a culture is of great importance in the engineering design of large installations. In a dense culture exposed to the sun, the cells near the surface will get too much light and use it inefficiently. Deeper in the culture, absorption will reduce the light intensity, and light will be used effectively. Still deeper, the cells will not get enough light to grow well. This state of affairs can be improved by stirring the culture to expose the cells alternately to bright and dim light.

Laboratory studies show that algae can use the full intensity of sunlight with the same efficiency that they use dim light, provided that the bright light is given in very short flashes, with the proper dark time between flashes. It is theoretically possible to get some 10 times the yield from an algal culture growing in sunlight if the culture can be properly stirred so that each cell receives the right pattern of flashing light. It has not been proved that stirring can produce such a light pattern. It may even be an engineering impossibility to do this in a large culture, and it is not even known how much benefit can be had from a type of stirring that is practicable. French and Davis were able to get a 70-percent increase in yield of *Chlorella* growing in bright light in a laboratory device especially designed to stir the culture violently.

At the Carnegie Institution of Washington we experimented with culture units of various shapes. We were principally interested in the effect of different cell densities and depths of culture on the yield of *Chlorella* grown in sunlight. Davis pumped a dense culture through coils of transparent tubing 7.5 mm inside diameter. Smith used a vertical glass column 13 cm in diameter. I experimented with two widely different sets of conditions. One was a low-density culture 30 cm deep in a 5-gal bottle. The other was 45 lit of very dense culture 17 mm deep over an area of 2.65 m^2. The same maximum daily yield from this shallow culture was obtained over a range of cell density from 2.7 to 11.5 g (dry wt.)/lit. All the cultures were stirred by pumping, shaking, or aeration. The most important observation was that, in each of the four containers, the yield of *Chlorella* was roughly the same per unit area of culture exposed to light, about 10 g (dry wt.)/m^2 day, or 16 tons per acre, per year. The yield of *Chlorella* per liter of culture was 6 times as much in the shallow dense cultures as it was in the low-density deep culture. Poor temperature control in these cultures may have prevented higher yields.

Later I grew *Chlorella* outdoors under thermostatic control. The yield was best with continuously maintained temperature between 20 and 25°C. This yield was increased 33 percent by using a 30° day and 20° night. Even in the short days and low light intensity of November, the yield was 18 g/m^2 day, or 29 tons per acre, per year. One would expect at least twice this yield in long summer days. A year-around average of 40 tons per acre is therefore not too wild a guess.

Turning from light utilization to other factors, one of the most important in mass culture of algae is an ample and inexpensive source of carbon dioxide. About 2 tons of it will be used for each ton of dry algae produced. Air contains only 0.03 percent carbon dioxide by volume. To grow a ton of algae using air, one would have to pump more than 4000 tons, about 110 million ft^3, through the culture. A little calculation shows that it would take all the energy released by burning the ton of algae just to pump this quantity of air through a 28-in. depth of culture. It is much easier to supply air containing about 5 percent carbon dioxide. This concentration was used in most of the experiments I have described. Davis found the same yield of *Chlorella* with 0.5 to 5 percent carbon dioxide, provided that the gas was bubbled through the culture fast enough to keep the concentration in the medium in equilibrium with that in the gas. Geoghegan found that carbon dioxide could be supplied to large cultures at either 5 percent or 20 percent with no difference in yield, if the rate of addition was adjusted so that about 85 percent was used by the algae. Perhaps even higher concentrations could be used with good results if the rate of addition were regulated to match the rate of use. It is known, however, that supplying even 10 percent carbon dioxide faster than the algae can use it is detrimental to yield. Conservation of

arbon dioxide indicates the use of a closed-culture system.

Large-scale outdoor culture of *Chlorella* in open trenches, to make use of the vast amount of carbon dioxide in the waste gases of the industrial Ruhr, was studied by Gummert, Meffert, and Stratmann in Essen, Germany. They used only 1 percent carbon dioxide in air. Some trouble was caused by contaminants in the cultures. The yields per unit area were about one-third of those from closed systems, and the efficiency of light use was correspondingly low, about 0.5 percent. From data given by the German workers, the production of *Chlorella* under unfavorable conditions during July to October was roughly 6 tons per acre, per year.

Workers headed by Tamiya in Tokyo have studied the growth of large *Chlorella* cultures outdoors. Their pilot plant had a concrete trough with a thin plastic cover and a dark tower in which the culture was aerated with 2.5 to 9 percent carbon dioxide. The culture was pumped through the tower and trough. Additional carbon dioxide-enriched air was led into the culture in the trough. In a 10-day trial run, the plant yielded dry *Chlorella* at a rate of 3.5 g/m² day, or 6 tons per acre, per year. The lower-than-expected yield was partly the result of unfavorable winter weather. The Japanese workers plan improvements in their pilot plant and also the construction of plants of different designs.

The much lower investment in open trenches might balance out low efficiency in use of carbon dioxide and make the system operable for commercial use. The absence of a cover would increase light and help cooling but at the cost of increased water consumption. These and other factors need to be studied by the comparative operation of open and closed systems.

The most extensive investigation of algal mass culture in this country was directed by A. W. Fisher, Jr., of Arthur D. Little, Inc., who operated a pilot plant at Cambridge, Massachusetts, for the Carnegie Institution of Washington. A study was made of the growth of *Chlorella* in types of equipment suitable for expansion to large-scale commercial methods. While data were being obtained on outdoor growth of algae, enough *Chlorella* was grown to permit some tests of its suitability for animal feed and for industrial raw material.

The first culture unit was made of polyethylene tubing 4 ft wide with 0.004-in. wall thickness, supplied by the Plax Corporation of Hartford, Connecticut. A 160-ft length of tubing held 1200 gal of culture in a layer 2 to 3 in. deep with about 600 ft² exposed to the sun. Air containing 5 percent carbon dioxide passed through the tube above the culture. A centrifugal pump circulated the culture and by-passed part of it through a heat exchanger for cooling. The *Chlorella* was harvested in a centrifuge. The yield averaged about 10 g/m² day, or 16 tons per acre, per year, during 3 mo operation.

What is most needed now is a full year's operation of a large pilot plant. This would give opportunity to improve engineering techniques, increase the yield of algae, and give an accurate evaluation of the cost of the product.

Next to water and carbon dioxide, the raw material needed in large quantity and at low cost for commercial culture of algae is fixed nitrogen. To get a ton of protein, one must put in about 320 lb of nitrogen in a form that the algae can use. One would use, for example, about 1.1 tons of potassium nitrate, or 0.75 ton of ammonium sulfate, or 0.34 ton of urea, or 0.20 ton of ammonia. The other mineral requirements of algae are not as heavy as the requirement for nitrogen. The selection and maintenance of a proper medium for a given alga in mass culture is an important chemical engineering problem. Economy in the use of water and mineral nutrients would dictate recycling of used medium after harvesting the algae.

Some of the difficulties in algal mass culture may be eliminated by finding new species to fit certain needs. For instance, the problem of cooling would be serious in a hot climate or where water is scarce. Myers and Sorokin have isolated strains of *Chlorella* that grow best at 39°C instead of at 20 to 25°C as ordinary *Chlorella* does. Such a strain should go far toward improving the water economy of algal culture. First attempts to grow the high-temperature *Chlorella* in quantity have not been as successful as expected. There are no doubt many other green algae suitable for large-scale culture. It is even possible that a better one than *Chlorella* may be found.

How would we use algae if we had tons of them at low cost? Some marine algae and their products have long been used for human food. So far, there has not been enough *Chlorella* to test its usability as a substantial part of man's diet. Tamiya says that *Chlorella* tastes like laver (a kind of seaweed) or powdered green tea, two favorite Japanese foods. Mrs. Tamiya has prepared noodles, bread, soup, and ice cream containing *Chlorella*. In quantities up to several percent, *Chlorella* is palatable in them.

Chlorella should be good food. Its fats do not differ greatly from some commonly used food fats.

It is rich in vitamins. Its protein contains all the amino acids known to be essential in nutrition. Feeding experiments using rats or chicks indicate that *Chlorella* protein is as good as the best plant proteins. Further evidence that green algae are good animal food is found in the success of the practice of fertilizing natural bodies of water to increase the algal growth to provide more food for oysters or fish. The algal "soup" produced by Jorgensen and Convit was cooked, and about a pint per person per day was fed to patients in the local leprosarium. The "soup" was palatable and nutritious. Jorgensen and Convit also claim that it had a curative value.

Besides use as food or feed, there are possibilities of using algae as industrial raw material. The fats of algae are similar to other highly unsaturated fats used in the paint and lacquer industry. Algae constitute a very rich source of such specialty products as chlorophyll, carotene, and vitamins. Other products, including sterols, may become important.

Algae culture should not be considered as a replacement of conventional agriculture but as a supplement to it. Algal culture can succeed where farming is impossible. No soil is required. It is adaptable to semiarid regions because of the low water consumption realizable by reuse of the culture medium. A continuous harvest is obtained instead of a once-a-year crop. Unless used on the spot, algae must be dried to permit storage or shipment without spoilage. To date, no biological bar has been found to algal mass culture. It seems almost certain that the experimental yields already obtained could be doubled or trebled by engineering improvements in control of the growing conditions. The potential value of algal culture is enormous.

Suppose that in the next few years algal culture begins to succeed as a means of supplementing mankind's food supply. As time and experience bring improvements, it may take over an even larger part of the task of feeding man. In a century or two, if algal culture follows the history of other great industries, it may reach a volume and efficiency hard to imagine at the start. Algae could even become inexpensive enough to burn for fuel. Perhaps by that time, when man's supply of fossil sunshine stored as oil and coal will be nearing exhaustion, he can turn to algae to keep his civilization running on the day-by-day supply of sunlight.

Greatness has always been a mark to aim at. In these rudderless days, when we are misguided by small and fumbling minds, it is not only inspiring but imperative to "think continually of those who were truly great." Soldiers on forgotten fields of battle, scientists in makeshift laboratories, stubborn idealists fighting to save a lost cause, teachers who would not be intimidated, tireless doctors, the anonymous army of dreamers and doers—all these by their very living fought for everyone. They sacrificed hours of ease for our casual comforts; they gave up safety for our security. Glorifying the heroic spirit of man, they added to our stature.—Louis Untermeyer.

Nutrient Supply for Large-Scale Algal Cultures

ROBERT W. KRAUSS

Dr. Krauss is research associate in plant physiology at the University of Maryland and research fellow of the Carnegie Institution of Washington. He was educated at Oberlin College, the University of Hawaii, and the University of Maryland. Since 1949, he has been studying the physiology of the algae with particular emphasis on their inorganic nutrition in mass cultures.

THE potential growth rate of any autotrophic alga is determined by its genetic constitution, but the actual, observable growth rate is a function of the physical and chemical environment in which the plant is compelled to exist. The actual will approach the potential if the environment is made to match the requirements of the species under culture. Meticulous delineation of these requirements is necessary if an adequate culture apparatus and medium are to be provided. Often the evidence is conflicting, and a full appreciation of the nature of the organism is difficult to achieve. Techniques apparently satisfactory for cell growth under one set of conditions may prove inadequate or even damaging under different circumstances. The concept of mass culture as a system for continuous, *efficient* utilization of solar energy has posed a challenge of a different order than was formerly met by phycologists in preparing cultures for taxonomic or physiological research. It is not sufficient to satisfy all the requirements for the initial period of growth; in addition, optimum environmental conditions must be maintained against the pressure of the growing cell population as well as against the thermal effects of insolation.

These culture requirements may be grouped under two headings: physical and chemical. One of the physical factors involved is the amount of light reaching each cell, which is conspicuously reduced as the population increases. A continuous harvesting program can prevent the amount of light from being reduced beyond a desired point. However the problem of determining the length of light and dark periods experienced by individual cells in a culture maintained at a given density is not so simply resolved (*1, 2*). The satisfaction of these requirements is a major consideration in the design of culture apparatus. Additional progress has been made toward both biological (*3*) and engineering (*4, 5*) solutions of the problem of high temperatures in cultures subject to solar radiation. The chemical requirements can be met by providing a satisfactory nutrient solution. Algae growing at an optimum rate because of a properly maintained physical environment will exert a maximum demand on the culture medium for the raw materials of metabolism. This paper (*6*) discusses the problems involved in providing and maintaining the chemical factors of the environment at an optimum level during continuous growth and harvest of mass cultures.

It is important to note here that the growth of higher plants, whether in soil or water culture, differs in several respects from that of algae in the continuous culture systems being considered. Not only is the total amount of plant material produced by the algal culture greater per unit of time and area, but also, because of the progressive harvest of a portion of the culture, any depletion of an essential element will be disclosed almost at once by a reduced growth rate. Although the higher plant's capacity for redistribution varies widely with the elements, periods of suboptimum supply can be tolerated longer with less visible damage. The increased susceptibility of the algal culture to depletion of raw nutrients is a function of more rapid growth, which in turn is made possible by both the higher level of CO_2 available and the harvesting regime, which permits optimum utilization of light. Neither the yields nor the problems can be compared directly with those encountered in horticultural hydroponics. The stringency of the demand on the nutrient solution will be a measure of the efficiency of the synthetic system obtained.

In 1949 a series of investigations was begun in the botany department of the University of Mary-

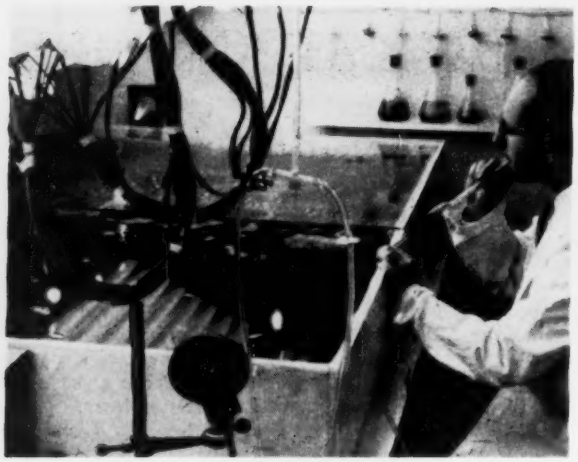

Fig. 1. Interior of the controlled-environment chamber showing one of the algal culture vats. The battery of lights is raised and part of the plate glass cover removed.

land to determine what factors might limit growth of algae in mass cultures. It was hoped that these investigations would provide a basis for increased understanding of growth under optimum conditions and also permit production of sufficiently large samples of homogeneous plant material for analytic study of metabolism in a controlled environment. Considerable information was available concerning both apparatus and nutrient mediums. Essentially two lines of attack have been employed in elucidating the nutrient requirements of algae. Small, discontinuous, or "batch," cultures, in which nutrient solutions of varying composition could be provided, have been used generally. Their relative efficiency can be evaluated by measurement of total growth after a period of time. Recently several workers, Ketchum (*7*), Scott (*8*), Myers (*9*), and others, developed continuous culture devices which permitted continued growth of the algae by introducing fresh mediums to the cultures wherever the cell density increased beyond a given point. Growth could then be measured either by increase in volume of the culture or by overflow. Mediums could thus be compared in cultures of algae growing at a rapid or an optimum rate.

However, it appears desirable to achieve continuous culture by a technique that would permit reuse of the medium after harvest—or, rather, a system for removing only the daily increment of algae from the culture. This would impose a requirement for replacement of absorbed nutrients at the same rate that they were removed in order to maintain the medium constant. Moreover, such a method would reveal any deterioration of the medium that could not be accounted for by exhaustion of nutrients. An interior view of the culture chamber built for these studies is shown in Fig. 1.

In order to obtain data from cultures illuminated by an artificial source as near to sunlight intensity and spectral distribution as feasible, batteries of combined incandescent and fluorescent lamps were constructed. Such a source provides illuminance averaging somewhat more than 1000 ft-ca at the surface of the culture. The algae are grown in polyethylene-lined, glass-covered, plywood vats having a surface area of 2 m^2 and a culture depth of 15 cm. The light source and the culture vessels are enclosed in an air-conditioned chamber. A 5-percent mixture of CO_2-in-air is continuously supplied to the cultures. Harvesting is effected by a Sharples supercentrifuge permitting return of the supernatant to the vat either directly or by pumping from a reservoir.

Experiments run in this apparatus are free of the shifts of light intensity and quality characteristic of solar radiation. Temperature also can be readily controlled and need not fluctuate. At the same time, the size of the cultures and the technique of harvest render information derived from them pertinent to mass culture apparatus that may be operated in the natural sunlight.

Scenedesmus obliquus, strain WH 50, which has a growth rate similar to that of *Chlorella*, was used in these experiments. The characteristic morphology of the organism makes contamination by other algae easy to detect. Furthermore, it seemed desirable to obtain information concerning another genus of the Chlorococcales with a growth rate great enough to be of interest. There is some reason to suspect that growth rates of different species, although disparate in dilute cultures where light is optimum, may not differ as greatly in mass cultures of high density (*10*). The morphology of *Chlorella* and *Scenedesmus* may be compared in Fig. 2.

The five inorganic macronutrients for algae have been determined to be nitrogen, sulfur, phosphorus, potassium, and magnesium. Only by the absence of calcium does this list differ from that for higher plants. Salts of these elements differing in variety and concentration, as well as those including sodium, calcium, and chlorine, have been used routinely in algal culture. The literature is replete with so-called "best" mediums for various species of algae. Often the superiority of a given formula has been a function of *p*H or CO_2 supply rather than the superiority *per se* of certain salts in specified ratios. Actually a number of different

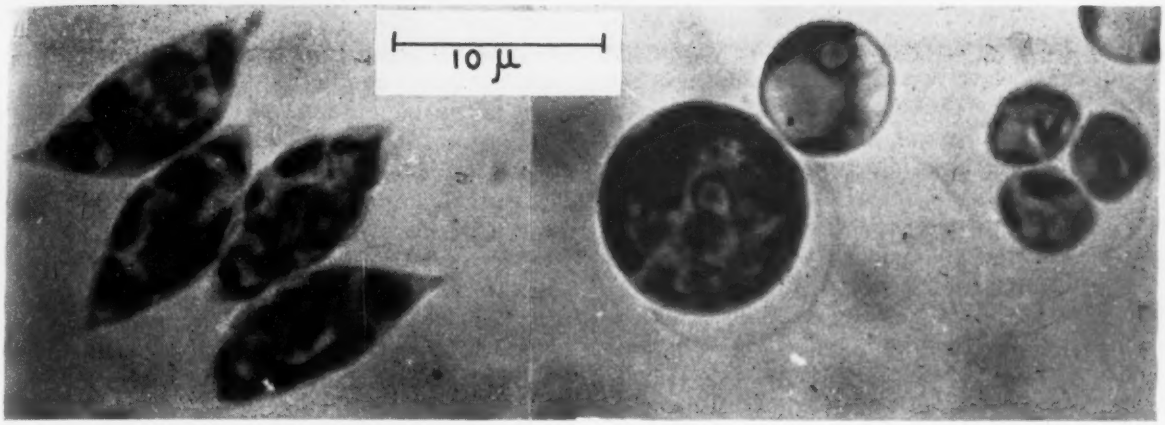

Fig. 2. Microphotographs of *Chlorella pyrenoidosa* (right) and *Scenedesmus obliquus* (left). A tetrad of young *Chlorella* cells can be seen at the right of the large adult cell.

mediums including the essential elements can be equally effective for the initial growth period. In most discontinuous cultures the exhaustion of nutrients, with the possible exception of nitrogen, is rarely the cause for cessation or diminution of growth.

In the selection of the nitrogen source, there is considerable latitude. The inorganic salts of nitrate including ammonium nitrate are similar in effectiveness. If ammonium salts other than ammonium nitrate are utilized, a progressive reduction in *p*H of the medium will occur; if nitrate is the nitrogen source, the *p*H will rise as growth progresses; and, if the two are balanced, the *p*H fluctuation is not conspicuous. In fact, a tendency toward either a rise or a fall in *p*H is not a serious problem, for it can be prevented by the periodic addition of nitric acid or ammonium hydroxide, and it serves as a useful index of nitrogen uptake. Moreover, the continuous supply of CO_2 provides additional buffering capacity because of the formation of carbonates and bicarbonates. A number of organic nitrogen compounds have been demonstrated to be nitrogen sources for green algae. Certain amino acids, amides, and urea serve as acceptable nitrogen sources for *Scenedesmus* or *Chlorella* (*11*). Urea has been shown to permit higher yields than inorganic sources (*5*). For the purposes of this investigation, however, inorganic nitrogen as potassium nitrate was used.

Preliminary analyses of algae and mediums suggested the use of a three-salt medium consisting of 1.0 g KNO_3, 0.25 g $MgSO_4 \cdot 7H_2O$, and 0.25 g KH_2PO_4 per liter. Periodic additions of nitric acid were used to maintain a high level of nitrate and a *p*H of 7.5. *Scenedesmus* was grown in this medium under conditions of continuous culture. Samples of the daily harvest were analyzed for inorganic components. Table 1 gives the maximum and minimum levels of the elements encountered in the algae during a period of 14 days. Analyses from cultures with and without added trace elements are reported. A detailed discussion of the shifts in composition of the algae grown under different micronutrient levels has been given elsewhere (*12*).

The problem of CO_2 requirement has been studied by Davis (*13*). The apparent optimum growth achieved by cultures supplied 5 percent CO_2-in-air mixtures is not a function of this high requirement *per se* but simply results from the difficulty in dispersing an excess of CO_2 in the culture to assure an optimum level for each cell. This level has been shown to be only slightly in excess of the 0.03 percent found in air. The maximum gaseous dispersal of CO_2-in-air in the medium is therefore desired. This can be achieved by increasing the surface-to-volume ratio of the gas bubbles. In our cultures this is accomplished by porous, carbon-pipe aspirators.

An alternative means of meeting the carbon re-

Table 1. Inorganic composition of *Scenedesmus obliquus* grown in continuous cultures.

Element	Micronutrients added (% dry wt.)		Micronutrients deficient (% dry wt.)	
	Max.	Min.	Max.	Min.
Mg	0.72	0.36	0.80	0.57
K	1.62	1.19	1.53	0.85
N	7.43	6.17	7.74	6.52
P	1.82	1.02	1.99	1.60
S	0.39	0.28	0.36	0.30

quirement would be the selection of an alga capable of utilizing bicarbonate as effectively as it can use gaseous carbon dioxide. *Scenedesmus* has been reported by Osterlind (*14*) to utilize bicarbonate. Some questions are still unanswered in this regard, but if an alga that possessed the facility for utilizing either dissolved CO_2 or bicarbonate could be used, the technical difficulties inherent in handling gaseous or liquid CO_2 would be diminished. Such algae may be found among those demonstrating rapid growth under alkaline conditions—a characteristic of many of the *Cyanophyta*.

Regardless of the ultimate method of providing the carbon source the 5-percent CO_2-in-air technique seems to be the most practical at this time and has been used in the studies discussed here. Depletion of certain nutrients in culture mediums may profoundly influence the biochemical composition of the algae. This has been well demonstrated by Spoehr and Milner (*15*) in their studies of the carbohydrate, fat, and protein content in *Chlorella*. The response of cells grown in deficient mediums may be of two types: (i) The fraction dependent on a given metabolite becomes limiting and the entire process of growth and development is halted. (ii) Only certain metabolic reactions are inhibited and the cells continue growth for some time—but at a diminished rate—either by sharing the limiting element or by developing a shunt metabolism of an extraordinary nature. This latter type has been nicely demonstrated by Finkle and Appleman (*16*) to occur in *Chlorella* grown under magnesium deficiency. *Chlorella* continues to grow, but division is halted and giant cells are produced. Similar effects have been noted in cells grown under micronutrient deficiency (*12*). A fruitful area of research is concerned with the shifts in organic composition that occur in cells grown under such abnormal conditions.

Data of Thomas and Krauss (*17*) pertaining to the change in the nitrogen fractions of *Scenedesmus* grown under different levels of nitrogen in the mass-culture apparatus are of interest in this regard. Algae harvested from a mass culture supplied adequate nitrate were placed in a second mass culture deficient only in nitrogen. As was expected, photosynthesis and growth continued. Consequently the nitrogen level of the cells fell from 7.5 to 2.0 percent of the dry weight in 5 days. We were interested in determining what happened to the nitrogen fractions in the cells during this period. Table 2 shows the analyses obtained.

Protein nitrogen, comprising 93 percent of the total nitrogen, was the major component at the beginning of the experiment. Progressive reduction of the total nitrogen content in the cells during 14 days of culture in the deficient medium was accompanied by an equivalent proportional decrease in free amino nitrogen, peptides, and basic nitrogen. There are, however, no major shifts in the proportion of the fractions, even though growth was progressing in an abnormal fashion.

A somewhat similar situation exists when the deficient cells are resupplied with nitrate. A total nitrogen level of 8 percent is attained within 24 hr. Soluble nitrogen (with arginine or an arginine peptide as a major component) reaches a peak after only 12 hr. and then diminishes as the protein fraction continues to increase. There is no major displacement in the composition of the algal cells during these extremes of sufficiency and deficiency insofar as the proportional distribution of nitrogen is concerned. This indicates that equilibration is strongly toward protein synthesis. Even under conditions of carbohydrate deficiency in the dark, only a slight increase in proteolysis was observed and little or no deamination could be detected. Further studies are underway to ascertain whether greater changes in the nitrogen fractions can be achieved by culture in deficient mediums or whether more drastic techniques employing metabolic inhibitors are required to effect a major shift.

Table 2. Changes in the nitrogen fractions (percentage of dry weight) of *Scenedesmus* in a nitrogen-deficient medium.

Fractions	Days in deficient medium									
	0	⅓	⅔	1	2	3	5	8	11	14
Total N	7.50	6.28	6.06	4.68	3.42	2.84	2.15	2.08	2.01	2.07
Protein N	6.95	5.88	5.65	4.36	3.22	2.65	2.03	1.95	1.87	1.95
Soluble N	0.55	0.50	0.41	0.32	0.20	0.19	0.12	0.13	0.14	0.12
Free amino N	.12	.10	.06	.04	.03	.03	.03	.03	.03	.02
Amide + NH_3 N	.01	.03	.01	.02	.01	.01	.01			.01
Combined amino N (peptides)	.05	.10	.11	.09	.09	.03	.03		.03	.02
Basic N	.05	.05	.06	.04	.03	.02	.02			.05

With the requirements for replacement of nutrients obtainable from calculations based on yield, we are prepared to examine the yields actually obtained from cultures in which sufficient replacement is made to permit optimum growth and yield. In any culture, increasing the density past a certain point will reduce the rate of growth. The point at which to harvest the increment for most efficient yield is at that level of cell weight where growth rate times cell density results in a maximum figure. This point—always at a level below the optimum growth rate—has been calculated to be at a cell density permitting a rate, $\overline{K}$, of 1 $\log_2$ unit or one doubling each 24 hr. The calculation is based on Blackman's formula:

$$\log_2 \frac{C_2}{C_1} = \overline{K}\ (t_2 - t_1)$$

The cell density (C) for the total culture of 300 lit at this point is 45 g dry weight. The culture therefore should produce approximately 45 g of algae per day. This rate of yield has been approached (42 g per day) only during the first days of harvest in cultures that were shown by analyses to be adequately supplied with all the major nutrients.

A tentative calculation of the efficiency of the process during the early days of harvest is instructive. Radiant flux measured by a thermocouple averaged 0.12 cal/cm² min over the surface of the vat at culture level with the plate glass cover removed. This is equivalent to 3456 kcal/vat day. If Wassink's (*18*) calculation for heat of combustion of *Chlorella,* 5.77 kcal/g, is similar to that of *Scenedesmus,* this would mean a net daily energy conversion of 248 kcal or an efficiency of 7.2 percent. If the entire apparatus could be expanded to cover 4046 m², or 1 acre, but under the same light conditions, the yield would be 85.0 kg/day or approximately 10 tons/100 days. Such extrapolation must be used with prudence but does serve for comparison with other large-scale apparatus. It must be kept in mind that the intensity of light in the controlled culture apparatus is less than one-tenth of that of sunlight but is continuous during the 24-hr day. It must also be remembered that data are not as yet sufficient to warrant confidence that this potential can be realized for any but a short period of time under the conditions of this experiment.

The causes for the later reduction in yield could be several. Because the introduction of additional complements of micronutrients increased the yield, it was felt that further study of the micronutrient requirements would be rewarding.

The essential micronutrients for algae are considered to be iron, calcium, manganese, zinc, and copper. Recently Walker (*19*) has reported that strontium may replace the calcium requirement in *Chlorella.* Walker has also reported a molybdenum requirement for *Chlorella.* Arnon (*20*) has shown an extremely small requirement for molybdenum and, under certain circumstances, for vanadium in *Scenedesmus.* A cobalt requirement for blue-green algae has also been reported by the Wisconsin workers.

In early work the trace metals were supplied as chlorides, nitrates, or sulfates. Recently Hutner (*21*) and Myers (*22*) have suggested and used the chelating agent, ethylenediaminetetraacetic acid (EDTA) as a means of maintaining the micronutrients available to the algae. This compound has been utilized to good effect in higher plants. Therefore cultures of algae supplied the micronutrients (except molybdenum) as chelates of EDTA were grown to compare yields with those supplied micronutrients as salts. The yield from the culture supplied EDTA was superior by 22 percent, even though five additional increments of micronutrients, each equivalent to one-fifth of the initial, were added, one every other day after harvesting began in the culture without EDTA. However, some reduction in the growth rate after the first days of culture was still apparent in both cultures. It seemed likely that even with EDTA there may be a failure to supply a sufficient quantity of micronutrients for optimum cell growth.

Analytic data giving levels of micronutrients to be expected in harvested cells were not available. Therefore rates of removal based on uptake and yield were unknown. Such information was secured from spectrographic analysis of cells from the first and tenth harvests from both cultures. These data are given in Fig. 3.

Interpretation of these preliminary data must be cautious, but certain points are obvious. Calcium and manganese were accumulated rapidly from both mediums by the algae. In the mediums without EDTA the metal content of the cells was greatly reduced. We cannot be sure, however, that these levels are low enough to be inhibiting, but this possibility is strongly suggested. The analyses for the other micronutrients in the cells grown without EDTA show no progressive diminution. Increased time for absorption resulting from the slower growth rate may be the cause for higher analyses in later harvests. The situation with regard to iron is not clear at this time. The level of iron in both analyses is high. Loss of citrate resulting in precipitation of iron in the one culture seems likely. Ob-

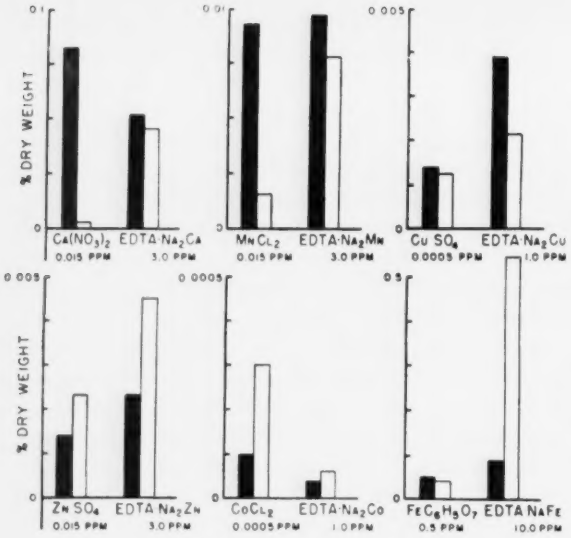

Fig. 3. The levels of trace metals found in *Scenedesmus obliquus* grown in mass cultures with the trace metals supplied in chelate complex or as salts. The sources of the trace elements are indicated by the chemical compounds named, and the levels of the trace elements in the mediums are given in parts per million (ppm). The solid bars give the analyses of the first harvest; the open bars give the analyses on the tenth day of harvest.

vious precipitation of the iron chelate, EDTANaFe, in the slightly alkaline solution has been observed in small cultures and almost certainly accounts for the high level in this case. With the exception of iron, it seems safe to say the EDTA formula has provided an ample supply of micronutrients. The special case of iron will be discussed later.

It is not possible, however, to be certain of the proper technique for replacing the metals absorbed from an EDTA medium even in view of the preliminary analyses obtained. Data obtained by Wallace (*23*) indicate that not only the metal but the entire EDTA complex is absorbed. This is an important point, for it would determine whether a replenishment of the metal to the solution will be sufficient, or whether it will be necessary to replace the metals in chelated form. We hope to attack this problem with EDTA labeled with C^{14} in the near future.

In view of the difficulty of calculating the amount of each metal in ionic form in a chelated medium comprised of many metals, and considering the uncertainty of the method of absorption from such mediums, an effort was made to determine pragmatically an optimum formula for chelated micronutrients. Experiments were run in a shake culture apparatus (*24*) permitting discontinuous culture of *Scenedesmus* in 500-ml flasks. This apparatus is shown in Fig. 4. The cultures were supplied with 5-percent CO_2-in-air and were measured for total growth after 10 days. Preliminary cultures had indicated toxicity when the total trace metal concentration exceeded 50 ppm. In each experiment the level of each metal, except the one under study, was kept at 1 ppm. In order to observe toxic effects, as well as to determine levels of sufficiency, each metal was tested at 0, 3, 9, 27, and 81 ppm. The results of these experiments are given in Table 3.

Although the purpose of the experiment was not to determine the essentiality of any micronutrient, double-distilled water, reagent-grade chemicals, and very small initial inocula of cells were used. Clear indications of deficiency are demonstrated for iron, calcium, manganese, and cobalt. The cultures without zinc and copper grew as well as the cultures that were supplied 3 ppm of these metals. A requirement for cobalt is indicated. Additional experiments designed to determine whether any other metals can satisfy this requirement have not yet been run.

It is recognized that more efficient growth might have been obtained in many of the cultures had a series of more dilute concentration been examined. This may be the case with zinc, copper, and cobalt. There is also the possibility that, if metals alone are absorbed by the algae, the relative increase in competition by the uncomplexed EDTA for other micronutrients might cause the very reduction in availability it was employed to prevent. This effect may account for the toxicity symptoms exhibited in higher concentrations. It is especially cogent with regard to the situation in the calcium series. The spectrographic analyses have shown calcium to be rapidly absorbed. A release of considerable amounts of EDTA might well occur. An overly efficient chelation could be just as troublesome as none at all.

In spite of the preliminary nature of the experi-

Table 3. Growth of *Scenedesmus* in flask cultures showing response to different levels of chelated micronutrients. The percentages given are averages of those obtained from duplicate experiments.

	Percentage of optimum concentrations					
Metal (ppm)	Fe	Ca	Mn	Zn	Cu	Co
0	01.0	72.0	18.7	100.0	100.0	63.5
3	95.0	100.0	87.5	100.0	100.0	100.0
9	100.0	64.0	100.0	73.7	89.5	69.5
27	36.5	38.9	62.5	79.0	79.0	61.5
81	0.0	27.7	6.2	84.2	68.5	55.0

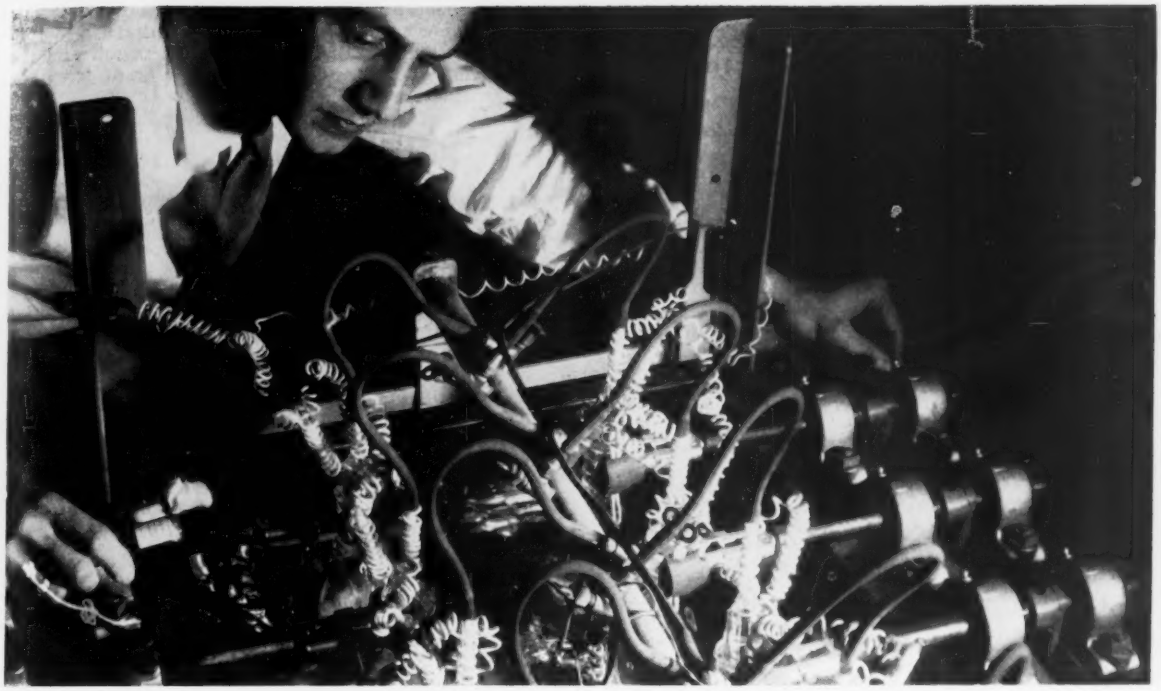

Fig. 4. A metabolic shaker for pure cultures of algae growing under constant temperature, light, and gas mixtures. [Courtesy *Baltimore Sun*].

ments, some idea of the best range of concentration for chelated micronutrients has been obtained. Levels of 9 ppm for iron and manganese and of 3 ppm for calcium, zinc, copper, and cobalt seem reasonable. Molybdenum should be included, of course, but the instability of its chelate complex with EDTA precludes its introduction in this form. At just what rate and in what form micronutrients must be returned to a continuous culture medium will depend on future experiments with the proposed formula.

A further set of experiments has been performed to ascertain the value of dihydroxyethylenediaminediacetic acid in maintaining iron in solution. The diol of EDTANaFe does not precipitate at an alkaline *p*H. Flask cultures employing this chelating agent for iron at 1 and 10 ppm showed no precipitation during the course of culture. Growth was similar to that in EDTA at 10 ppm but 10 percent greater in the diol at 1 ppm. It is highly probable that the diol will prove more useful than EDTA in mass cultures where a temporary *p*H rise may occasionally occur.

The effectiveness of the formula for chelated micronutrients has not yet been tested in mass cultures (*25*). The final answer to the question of whether micronutrient deficiencies are primarily responsible for reduction in yields cannot be given at this time. Nor can we be sure that some other chelating agent or even the metal-containing-glass or glass frit used for higher plants could not be employed to better effect. However, we are approaching a technique that should give us reasonable assurance of a micronutrient level sufficient for optimum yield. If we are still faced with the problem after guaranteeing a medium capable of supplying the nutrients we can look elsewhere for the cause. The likelihood of autoinhibitors is not to be overlooked, even though past evidence indicates that none accumulate. The possibility of deleterious effect of bacteria or fungi in the medium has also been considered (*12*). It does not seem likely at this time that the saprophytic bacteria present in such cultures damage yield. Nevertheless, in anticipation of a phycopathological problem, a screening program has been instituted for the purpose of finding bactericides and fungicides that are nontoxic to algae. In brief it may be said that our success thus far in increasing the yields of cultures by improvement of micronutrient medium leads us to believe that this may be the major limiting factor in cultures of this type.

An understanding of the total nutrient requirements of algae to be grown in mass culture is basic to the problem of securing continuous optimum

yields and efficient utilization of solar energy. The problems discussed in this paper present but a fraction of the total confronting the investigator. Consideration of culture of algae in saline or sea water presents many more complicating factors that will demand refined techniques for solution.

Nevertheless, the growth of any alga will depend on a balanced supply of the required nutrients. Even in complex organic mediums with countless compounds in solution, it is entirely possible that growth may be limited by the absence or unavailability of some element generally thought to be sufficient. It may not be too rash to suggest that some of the remarkable fluctuations in microorganisms—particularly algae—observed in natural waters may be in response to increase or decrease in the availability of micronutrients. It would be surprising indeed if the knowledge we are accumulating by magnifying normal requirements for growth in mass cultures did not provide insight into the many natural phenomena that have been so perplexing for so long.

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Rowland of Johns Hopkins once met one of his former students and asked him whether he was doing any original work. "No," was the reply, "I haven't any time or any money." Rowland shook his head; "Don't need any time, don't need any money, if you've got the will."—E. H. HALL.

Organism and Machine

These three articles—by Wolfgang Köhler, N. Rashevsky, and Warren S. McCulloch—are based on papers read by the authors as part of the symposium on "Organism and machine" that comprised the fourth part of the five-part conference on the general subject Validation of Scientific Theories *held in Boston, Massachusetts, in December 1953. George Wald of Harvard University was chairman for the symposium.*

The papers for the first three parts, on "Reasons for the acceptance of scientific theories," "The present state of operationalism," and "Psychoanalysis and scientific method," appeared in the September, October, and November issues, respectively. Those for the last part, "Science as a social and historical phenomenon," will appear in a subsequent issue.

Direction of Processes in Living Systems

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OUR topic "Organism and machine" is clearly a short form of the following question. Can the functioning of organisms be explained in terms of the conditions and actions which are found in inanimate machines? It might at first appear that this question ought to be answered by experts in biology, namely, by anatomists, physiologists, and biochemists. Unfortunately, these experts are not inclined to deal with such general issues. If we were to ask a man who is now studying, say, the role of sodium ions in the transmission of nerve impulses, he would probably tell us to leave him, please, alone—that he has no time for speculation. Who would not sympathize with this scientist who likes to work on problems for which precise solutions can probably be found in the near future? On the other hand, specialists should not criticize us too severely if we are interested also in more general issues. For, if the behavior of sodium ions in the active nerve fiber were perfectly known, if we had discovered the last vitamin, and so forth, we should still have to ask why, taken together and interrelated, the various operations of the organism tend to preserve its existence as well as they do. Can this achievement be explained in terms of machine conceptions?

Actually, the philosophers of science and the theoretical physicists may be at least as competent to clarify this issue as are the specialists in biology. I shall now try to indicate how these people might approach our problem.

The tendency of organisms to maintain themselves by their own processes is too obvious to need illustrations. But how are we to decide whether this tendency can be explained in machine terms, if we are not sure what we mean by a machine? Is just any part of inanimate nature a machine? Sometimes we talk as though we used the concept in this extremely wide sense. Even the physical universe as a whole may occasionally have been called a machine. The trouble is that the same term has also a much more specific meaning, and that, when discussing the topic "Organism and machine," we are for the most part not aware of this ambiguity. A machine in the more restricted sense is a physical system in which rigid arrangements or constraints compel events to take a certain course. In a well-constructed machine, this influence is one-sided. The constraints of the machine exclude all possibilities of action which would not be in line with the intended course; but, typically, the constraints cannot be altered by forces which action exerts on such solid conditions.

A simple example will make this clearer. One can easily compel an electric current to take a course which has the shape of a *W*. For this purpose, it will suffice to conduct the current through a rigid wire, part of which has this shape, and is kept in this shape by being firmly attached to a suitable support. There is nothing in a current as such that favors this particular shape. The form of a *W* is impressed upon the current only by the

described arrangement. It is in this fashion that physical events are forced to follow prescribed ways in man-made machines.

We must next show that the form which a physical process assumes can also be determined in an entirely different manner. Take this example. A thoroughly flexible insulated wire which forms a closed curve is placed on a smooth and plane surface. At first, the curve may be given any arbitrarily chosen shape. This shape will at once be altered if now an electromotive force is induced in the wire so that a current is set up. The shape is changed by the magnetic field of this current, and the direction of the change is such that it enlarges the area surrounded by the conductor. Actually, the conductor may be transformed into a circle, the shape in which it circumscribes the greatest possible area.

The difference between this and the preceding example must be obvious. In the present situation, there are no particular constraints by which a special form of the conductor and the current is prescribed. Rather, it is the free dynamics of the system which brings about the change and determines its direction. Obviously, then, we have good reasons for distinguishing between these two factors on which the form of physical events in a system may depend: constraints, on the one hand, and directions inherent in the dynamics of the system, on the other hand. In a given instance, both factors may, of course, operate at the same time.

When trying to understand phenomena which exhibit a striking order or show a persistent direction, man thinks more readily in terms of constraints by which such facts might be explained than in dynamic terms. Thus the remarkable order of movements in the translunar world was once explained by crystal spheres to which the stars were supposed to be attached. In this fashion they had to perform prescribed movements. Again, when Descartes tried to explain the order of organic processes, he began at once to think in terms of anatomical arrangements which enforce this order. It never occurred to him that, quite apart from such arrangements, directions inherent in biological dynamics might play a major part in the self-maintenance of living systems. Even in our time, it seems sometimes to be felt that, when the dynamics of a system is allowed to operate on its own, the result is likely to be chaos.

In typical inanimate machines, operations are constrained to take courses which serve human purposes. But systems may have the characteristics of machines without having been built by man. The crystal spheres of Aristotelian astronomy, for instance, were assumed to be of divine origin; and Descartes probably believed that the anatomy of organisms had been chosen by the Lord, who in this fashion made it possible for them to maintain themselves. In our time, evolution has taken the place of such agents. Evolution has, of course, no interests and no purposes. Nonetheless, if we follow Darwin, living systems tend to change in a particular direction, the direction in which their chances of survival are increased; for, those that do not so change will soon succumb in the general competition inherent in living. But this does not explain the changes as such. What is their nature from the point of view of science? Darwin did not explain his theory in specific physical terms. In this respect, his work does not entirely satisfy the theorist of our time. However, his statements clearly imply that evolution changes mainly anatomical conditions; in other words, that it builds better and better arrangements by which organic events are forced to occur in biologically useful directions.

In this assumption, only one point seems to me open to criticism. Anatomical arrangements as such produce no action; they merely modify actions which occur for dynamic reasons. Consequently, the principles of dynamics cannot be ignored in a theory of evolution. But, actually, they are almost being ignored in this particular theory. Does the general principle of evolution contain any implications concerning the dynamics of organic processes? There is at least one such implication. In a strict theory of evolution, it must be assumed that evolution has changed no law of dynamics, and that it has introduced neither new forces nor new elementary processes. In this respect, the principle is therefore one of invariance rather than of change. Hence, in considering any *events* which occur in organisms, we have to think, first of all, of the laws of dynamics which are here involved. Presumably, these laws are still the same as they were before organisms appeared on this planet. Only as a second step can we then proceed to examine the particular conditions under which such laws now operate in given living systems.

Nobody will deny that, on the whole, histological facts serve to give organic events a useful form. The anatomy of the human eye, for instance, can hardly be described without referring to the use of this structure in clear visual perception. So numerous are examples of this kind that, without any doubt, organisms may to a large extent be regarded as machines in our more restricted sense of the term. Are we to conclude that living systems can be entirely explained in this fashion? Clearly, no such conclusion would be justified, because it can

be shown that organisms maintain themselves, not only because of useful anatomical conditions in their interior, but also for reasons inherent in the principles of dynamics.

In the *first* place, we must remember a simple biological fact. It is true that certain anatomical arrangements force processes to take a course which helps the organism to survive. But what is the nature of such arrangements? Are they really comparable to the rigid constraints which we find in inanimate machines? In a most important sense, they are not. The very way in which they exist differs widely from the way in which constraints exist in machines. For the most part, such constraints consist of solid objects; they are composed of permanent materials which have been given one shape or another depending upon their particular purpose. On the other hand, no part of the anatomy of an organism is a permanent object. Rather, any such part must be regarded as a steady state, only the shape of which persists, while its material is all the time being removed and replaced by metabolic events. Surely, such steady states are fairly resistant and can, therefore, serve as constraining devices for more temporary functions. But from the point of view of our general topic, it is a most important fact that all organs, large or small, are processes rather than permanent objects. For it follows that any question concerning the useful course of those temporary functions must now be asked again with reference to the anatomical arrangements by which this course is enforced. If these arrangements are actually also processes, why is the form of *these* processes maintained for long periods, as it must be if the organism is to survive? It will hardly be suggested that the shape of the anatomical parts under consideration is maintained because there are further anatomical constraints which force metabolic events to operate in this fashion. For, what has just been said about the former anatomical arrangements would at once have to be repeated with regard to such hypothetical arrangements of a second order. They, too, would surely be steady forms of processes, rather than solid objects, and would therefore be subject to the same reasoning. Anybody who is familiar with the history of science will admit that, when theories take such a turn, something is probably wrong with basic premises.

At this point, some people take refuge in vitalism. I am not inclined to do so. It seems preferable now to consider a *second* reason which is opposed to an interpretation of organic events in machine terms alone. This reason is derived from physics. The organism would be a machine in the strict sense only if the behavior of *all* its parts were prescribed by special anatomical devices. Now this is clearly not the case. It will suffice if we mention one example. The distribution of the tissue fluid which pervades all parts of the body is not determined by particular devices. This continuum has, as the physicists would say, innumerable "degrees of freedom." To a large extent, its distribution is therefore a matter of dynamics. It follows that, if nevertheless this distribution tends to be favorable to survival, the self-maintaining conduct of the organism must be derived partly from a direction inherent in dynamics. Since the same consideration holds for other parts of the organism, principles of dynamics obviously play an important part in its self-maintenance.

This is my main point. From here, we should, of course, proceed to a thorough examination of dynamics in general, in order to discover what directions it takes in systems of one kind or another. Unfortunately, knowledge in this field is so restricted that only a few remarks can be made at the present time.

What we learn from the physicists in this respect may be formulated as follows. If, in a closed system, macroscopic velocities are constantly being destroyed by friction, transformations in the system will be such that the sum of all energies capable of producing further transformations decreases. When these energies have reached a minimal amount, the system will no longer change. Although this is a perfectly good principle, it cannot, in its present formulation, be applied to the organism. For the organism is obviously not a closed system; moreover, while the direction indicated by the principle may be called "downward," the direction of events in healthy organisms is on the whole surely not "downward" but, in a good sense, "upward." Thus the energy content of a young organism generally increases, and when at times it spends more than it gains, events soon take a turn by which the loss is balanced, if not overbalanced. Nevertheless, no serious difficulty arises at this point, because the direction which events take in open systems need not be the same as that which they take in closed systems. In fact, events in some simple physical systems tend to develop "upward," just as does the energy content of a young organism. For instance, any fire which is locally started, and then grows or spreads, exhibits this behavior. It can do so because the first weak flame is in contact with combustible material and with the oxygen of the surrounding air, which together constitute a store of potential energy. In other words, the flame is an open system, and the

closed system which must here be considered contains in addition this source of energy. For the closed system as a whole, it remains true that it must develop "downward." But it does not follow that the same must happen to the part of it in which we are interested, namely, the flame. For as soon as the fire is started, it begins to feed on the store of potential energy, so that its own energy grows.

It is quite possible that the same principle is involved when a young organism develops "upward." For, this organism is also an open system surrounded by stores of energy which it absorbs and spends in growing. Taken together, the food which it eats and the oxygen which it inhales constitute large amounts of energy. Only one point remains to be added. When a flame, say, that of a candle, has developed to maximal size, it maintains itself at this level so far as it can. Need we be surprised if, once an organism is fully developed, it maintains its vigor so far as circumstances permit?

I must confess that, although this reasoning may point in the right direction, it is, for my taste, too abstract and general. There are too many problems to which it does not refer at all. For instance, the fact that organisms are open systems does not protect them against illness or old age. In both situations their energy tends to decrease. Moreover, sooner or later organisms die, even if they are not injured by outside agents. This means, of course, that open systems *may* develop "upward," that they *may* maintain themselves in states of high vigor, but that they need not, and do not, do so under all circumstances. What, then, are the conditions under which the present principle works? When, on the other hand, will an open system deteriorate even though energy can be transferred to it from an outside store? So long as we cannot answer such questions, we are far from understanding the way in which living systems maintain themselves. As a further criticism, I should like to point out that between certain simple systems in physics, which develop "upward," and the organisms with their tremendous variety of operations there still remains an enormous gap. As a result, the behavior of the former does not help us very much when we try to clarify that of the latter.

Such shortcomings may, of course, be remedied in the future. Many physicists are now strongly interested in biological questions. It should not be difficult for them to discover under what circumstances open systems of the inanimate world show the "upward" trend, and under what others they do not. Moreover, such physicists might also investigate open systems which are not so simple as the flame of a candle. The most astounding characteristic of organisms is the fact that so many processes in their interior virtually seem to cooperate when, as a whole, they maintain the organism's existence. Let us hope that the problem which arises here can be solved with the conceptual tools of physics. But it must be determined that at the present time the task still seems enormous. Even macroscopic physics is not yet a completed science. It has hardly begun to study the behavior of open systems.

Is the Concept of an Organism as a Machine a Useful One?

N. RASHEVSKY

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HALF a century ago scientists used to evaluate their theories and theoretical concepts by asking the question: Is the theory or theoretical concept true or false? Since the days of Henri Poincaré the notion has gradually gained general ascendancy that the aforementioned criterion is not the proper one. In fact it is not even a meaningful criterion. Nowadays we do not ask whether a given theory or concept is true or false. We ask: Is it convenient or inconvenient: is it useful or not? The word *useful* is meant here not in the ordinary practical, utilitarian sense, which may enable the proverbial man in the street to translate it directly into dollars and cents. The convenience or usefulness of a theory or theoretical concept is measured by the savings it effects in our

mental efforts, which are needed to correlate the theory with experience. It is the "economy of thought" that constitutes the measure of the usefulness or convenience of a theory.

To give an example, let us consider a generally known case from the history of physics. Even now, if one choose stubbornly to do so, it would be possible to explain all known physical phenomena from the point of view of the old concepts of absolue space, absolute time, and absolute motion. This can be done, provided that we are willing to pile up one *ad hoc* hypothesis upon other *ad hoc* hypotheses. But, except for velocities that are negligible as compared with the velocity of light, we would obtain a theoretical mess that no scientist could comprehend.

On the other hand, if we accept Einstein's ideas of relativity of space, time, and motion and use the concept of a four-dimensional space-time manifold, a number of complex phenomena receive a simple and mathematically elegant explanation. Each physcial phenomenon falls, so to speak, naturally into its proper place. The economy of thought achieved is enormous. This is why we consider the classical concepts of space and time as generally inconvenient and useless, whereas we consider the relativistic concepts as very convenient and very useful.

There is another important criterion by which the contemporary scientist judges the usefulness or convenience of a theory or theoretical concept. A theory or theoretical concept is considered the more convenient or useful, the better it enables us to predict facts that hitherto have not been observed or that may have been observed but escaped notice. The history of the natural sciences is replete with such predictions. To mention only one, we can again refer to Einstein's theory of relativity, by means of which he predicted the deviation of light in the gravitational field of the sun, and the equivalence of mass and energy. The practical importance of the latter prediction does not require any discussion.

It is natural to ask the questions: How does the scientist make these predictions? What is it that sometimes gives to the scientist these peculiar oracular or prophetic powers? A great poet thought he could find the answer to this question in a highly poetical way. In Schiller's words:

> Mit dem Genius steht die Natur im ewigen Bunde;
> Wass der eine verspricht, leistet die andre gewiss.

As scientists, however, we can hardly be satisfied with this poetical explanation, and we must seek one that is more prosaic but also more scientific.

If we examine carefully the processes by means of which various predictions in the natural sciences have been made, we find that their mechanism works in the following manner. The scientist constructs theories, theoretical concepts, or theoretical frames of reference that are isomorphic with the world of observable phenomena. This isomorphism is never complete, never covers the whole range of observable phenomena. A complete isomorphism is the dream of the scientist. Whether this dream will ever come true is a question that I shall not discuss now. But the wider the range of isomorphism, the greater the predictive value of the theory. The isomorphism assigns, within a certain limited range, to each element of the conceptual framework a particular observable phenomenon, and to each relationship between the elements of the concept a particular relationship between the corresponding observable phenomena. The theory is "tested" by first verifying the correspondence between the elements of the conceptual framework and the *actually observed* phenomena. If the range of isomorphism exceeds the range of the phenomena actually observed, then the scientist can make predictions. He notices that in his conceptual structure there are elements to which no corresponding phenomena have yet been observed. Assuming that the isomorphism extends beyond the range of phenomena already observed (and this is a prerequisite of a good theory), he concludes that there must be in the surrounding world phenomena not yet observed and, from his conceptual framework, he deduces the relationships between those as yet unobserved phenomena. Within the range of isomorphism his prediction of new phenomena will come true. Outside of that range his predictions will fail.

The situation may be illustrated by means of a crude and inadequate example, which, however, brings home the essential point. Most of you doubtless have seen various ancient geographic maps, for instance, maps used by the ancient Greeks or Romans. The general outlines of the Greek peninsula and of the Italian "boot" were fairly accurately represented; to a lesser extent were the outlines of the North African shore; still less accurately, the outlines of the western Mediterranean. Beyond those limits many ancient maps are ludicrously fantastic.

A map is good and useful only when it is isomorphic to the actual terrain that it symbolically represents. Thus within the relatively small range, confined to the Greek and Appenine peninsulas, those ancient maps were fairly good. An individual who never traveled before and who set forth on a long journey could, with the use of such a map, find out fairly accurately what type of terrain he

would meet on his travels. In a very broad sense, he thus could "predict" some of his future experiences. But this would be possible only provided that he did not travel too far beyond Greece or Italy. If he attempted to circumnavigate Africa or travel into the Baltic Sea, his map would be utterly useless, and he would be up against all sorts of "unpredicted" surprises.

It should not be thought that the conceptual structures of the scientist can represent only strictly causal relationships. They can just as well represent the probalisitic relationships that are so frequently observed in modern physics and in other natural sciences. Instead of being characterized by the statement: "Element *a* stands in such and such relation to element *b*," the conceptual framework may be characterized by the statement: "The probability that element *a* stands in such and such relation to element *b* is so and so." An excellent example of this type of conceptual framework is offered by the theory of random nets, as developed by Anatol Rapoport and his students in connection with their studies in the mathematical theory of the brain.

Thus the discovery of conceptual structures that are isomorphic to the observable world is the prime function of the theoretician. It was the great achievement of Einstein to have noticed that physical phenomena are within a wide range isomorphic to geometric relationships in a four-dimensional non-Euclidean hyperspace.

Now let use come to the subject of this discussion: the organism and the machine. From the old point of view, we could have asked the question: Is the organism a machine or not? That such a question is meaningless can be particularly well seen in this example. The answer to the question depends on the definitions of the word *organism*, and *machine*. I submit that organism has not yet been defined in a way that satisfies all the criterions of a good definition given in the textbooks of logic. Doubtless we shall all agree that everyone of us is an organism. If this were summertime, I might notice on a window a crawling fly or a mosquito. Again we will all agree that this is an organism. A plant in a flowerpot on the windowsill also is undoubtedly an organism. We also would all agree that the mildly pathogenic bacteria deep down in my throat, which cause me to cough occasionally, are organisms. But when we come to viruses, opinions divide, and there is still no general agreement on whether or not they are organisms. I submit that this lack of agreement is an indication of lack of a proper definition of an organism.

Now let us turn our attention to the concept of machine. Most people understand by machine a man-made contraption, which performs certain functions that serve a *preconceived purpose* of its inventor or maker. Among the Greeks there were those who defined a machine somewhat differently, speaking of a contraption of divine creation. In any case a personal purposeful creator was implied.

In the light of these definitions, let us now examine what we should do to answer *scientifically* the question of whether an organism is a machine. I insist on the word *scientifically*, meaning that we should use only evidence that withstands the acid test of scientific reliability. It is clear that we must ascertain, *by scientific methods*, the existence or nonexistence of a personal creator for any organism. I submit that if we ever even begin to follow this path of inquiry, we shall have left the domain of the scientific before anyone can say "Jack Robinson." Thus in this case, as in all others, the old approach is shown to be useless and even meaningless.

From the point of view of the contemporary approach we may, however, legitimately ask the question: Is a particular, specified organism, or a clearly specified part thereof isomorphic to a given specified machine? This question is meaningful and can be answered either in the affirmative or in the negative. We shall now show by several examples that *within a small, sharply circumscribed range*, the answer to the question is *Yes*. We shall also see that therefore, *within that range*, the concept of a machine as a structure isomorphic to certain organisms, or parts thereof, is useful.

A crude analogy has been noticed between the heart and the vascular system on one hand, and a pump and system of pipes on the other. On the basis of this crude analogy some equally crude quantitative relationships have been deduced. Crude as they are, they have been very roughly confirmed by experiment. With the refinement of the mechanical analogy and with the complications of the conceptual picture, more refined relationships have been derived, and the agreement between theory and experiment has been considerably improved.

A particularly interesting example is offered by the development of various electronic computors. These machines perform complex tasks that hitherto have been the function of certain parts of our brain. Thus we may say that the electronic computors are, *with respect to their functions*, isomorphic with a small part of the human brain. We may legitimately ask whether this isomorphism extends also to *structure*. The answer to this *in general* is *No*, because neither an electronic tube nor

a transistor is structurally isomorphic to a neuron. In *some* structural aspects the answer may well be *Yes*. It is quite possible that in the topology of the interconnections between different elements of a computor and different elements of the brain, an isomorphism, or rather a homeomorphism, is at hand. Studies in the mathematical biology of the central nervous system have sometimes proved useful in the design of some machines. Conversely, the designers of the electronic computors may give some highly useful suggestions to the mathematical biologists.

The third example is from a field of mathematical biology to which I have made a modest and rather inadequate contribution. I have in mind the theory of form of plants and animals, considered as optimal structures for the performance of given biological functions. For example, by considering the trunk of some quadrupeds as a beam supported at its two ends, we can derive from this mechanical analogy some approximate relationships between the gross length and width of the trunk and its mass. Such relationships have been found to agree roughly with actual data. Other mechanical analogies lead to expressions between the mass of the heart and the heart frequency, and the total mass of the animal. Analogous expressions have been derived for the size of lungs and the frequency of breathing, as related to the over-all mass of the animal. They all agree roughly with the data that are available.

One of our brilliant students, David Cohn, has recently applied this approach to the structure of the vascular system. He has derived theoretical relationships that determine such quantities as the diameters of the main arteries, the average number of branchings in different size arteries, the average number and size of capillaries. The agreement between the theoretical expressions and available data is quite remarkable.

Thus we see that the notion of ismorphism between certain parts of some organisms and definite mechanical or electromechanical structures may be quite useful. How far should this approach be pushed? Only the future can tell. My personal feeling, and this is nothing but a feeling, with which anyone is just as likely to agree as to disagree, is this: Although in certain parts of biology, as, for example, some aspects of form, the mechanical analogies are likely to prove very fruitful, I do not think that the future of biology lies in too strong an emphasis of the analogies between organisms and machines. Some of the reasons for this feeling are the same as have been discussed by Wolfgang Köhler. It also seems to me that if we are to map successfully complex organisms upon some complex electromechanical structures, we will have to be guided in our design of those structures by our biological knowledge, thus creating a logical circle. To whatever concept biological phenomena will be found to be isomorphic, I do think it will be different and more general than that of a machine. Biology is still awaiting its Einstein, who by a stroke of genius will map the complex organismic phenomena onto a known physicochemical, physicomathematical, or purely mathematical structure.

Mysterium Iniquitatis of Sinful Man Aspiring into the Place of God

WARREN S. McCULLOCH

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D'ARCY THOMPSON used to tell of his encounter with a biologist who had described a nearly spherical diatom bounded entirely by hexagons thus:

"But," I said, "Euler showed that hexagons alone cannot enclose a volume." To which the innominate biologist retorted, "That proves the superiority of God over mathematics."

Euler's proof happened to be correct, and the observations inaccurate. Had both been right, far from proving God's superiority to logic, they would have impugned His wit by catching Him in a contradiction. Our first concern is to avoid the impropriety of such solecisms.

Our second resembles it slightly. Newton, Jeans, and Planck have used "God" to account for things they could not explain. Biologists, ignorant of mechanisms underlying functions, have introduced "Nature," "Vital Force," "Nervous Energy," "the Unconscious," or some other pseudonym for God. Each of these supposititious explanations, to quote Sir Thomas Browne, "puts the honest Father to the refuge of a miracle."

Today no biological process is fully understood in terms of chemistry and physics. The facts are unknown to us. Few chemical properties are yet reduced to the physical relations of atomic constituents. The mathematics is too cumbersome. Physics itself wants a unified field theory and doubts determinism in atomic processes.

So much for Comte's hierarchical unity of science! At last we are learning to admit ignorance, suspend judgment, and forego the *explicatio ignoti per ignotium*—"God"—which has proved as futile as it is profane. Instead we seek mechanisms, for two purposes.

Let us consider them one at a time. As soon as we devise a machine that will do what has to be explained, we divest the superstitious of any seeming warrant to his miracle. It is enough to show that, if certain physical things were assembled in a certain way, then, by the laws of physics, the assemblage would do what is required of it. So imaginary engines led Carnot to entropy and Maxwell to his electromagnetic equations, instead of to miracles. Both machines, to their inventors, were more than metaphors for mathematics. But actual engines proved Carnot's a homolog, whereas the elastic ether's being chimerical left Maxwell's a mere analog. Yet, because each showed there could be a machine that turned the trick, it were best to see them—at least from the logical point of view—as existential devices.

By these means biologists have exorcised ghosts from the body, whence they went to the head, like bats to the belfry. To drive them thence, my mentor, Pike, spent his life replacing them by simple engines to ring all the changes on the chimes. He looked on the evolution of the nervous system, on its ontogeny, on learning, even on reflexes, as spontaneous variants that survive in the competition to trap available energy and thus secure energetic *Lebensraum* in the entropic degeneration of sunshine to the *Wärmetod*. Whether atomic or molecular chaos produces a sport, its thanks are due to chance, not to divine intervention in its behalf. These notions do not constitute mechanistic hypotheses but exhort us to construct them. Call them metaphysical if you will—in this good sense, that they prescribe ways of thinking physically about affairs called mental and relegated to the whims of spirit manifold. I am of Pike's persuasion.

But most people have heard of cybernetics from Norbert Wiener or his followers. Narrowly defined it is but the art of the helmsman, to hold a course by swinging the rudder so as to offset any deviation from that course. For this the helmsman must be so informed of the consequences of his previous acts that he corrects them—communication engineers call this "negative feedback"—for the output of the helmsman decreases the input to the helmsman. The intrinsic governance of nervous activity, our reflexes and our appetites exemplify this process. In all of them, as in the steering of the ship, what must return is not energy but information. Hence, in an extended sense, cybernetics may be said to include the timeliest applications of the quantitative theory of information.

The circuit in a servomechanism may include, as we hold it does in man's head, complicated machines of calculation. Turing showed that one having a finite number of parts and states, scanning, marking, and erasing one of four symbols at a time on an infinite tape, can compute any computable number. The first part of the tape serves to prescribe which number his general machine shall compute. Pitts and I showed that brains were Turing machines, and that any Turing machine could be made out of neurons. For this we used a calculus of atomic propositions subscripted for the time when all-or-none impulses signalized them in the relays constituting the net, or the machine. In brains the relays are neurons and the blueprint of the net is the anatomy of their connections.

Since Hilbert arithmetized logic, the calculation of any computable number is equivalent to deducing any conclusion that follows from a finite set of premises, or to detecting any figure in an input, or to having any general idea that can be induced from our sensations. Existential operations can be introduced into our calculus by inserting in the net any circuitry that will secure invariants under groups of transformations. Memories, general ideas, and even Spinozistic consciousness, the idea of ideas, can thus be generated in robots. These robots, even simple ones having but half a

dozen relays, may, without inconsistency, show that circularity of preference, or of choice, called the value anomaly which—contra Plato—precludes a common measure of "the good."

Elsewhere I have shown that computing machines by playing chess may not merely learn to play better than their designers, as Ashby would have it, but that they may learn the rules of the game when these are given only ostensively. This insures their ability to generate their own ethic—not merely to be good, like the virtuous savage, because they are so made that they cannot break the rules, nor, like the gospeled or inspired, because they were so instructed by their fellows or their creator. Unlike solitaire, chess can be enjoyed only by a society of men or machines whose desire to play exceeds their desire to win. This is easily determined by connecting their two feedback loops in such a way that the former dominates the latter. I grant that these complicated machines resemble the elephant or some other "Colossus of Nature" rather than ants, within whose "narrow engines there is more curious mathematic; and the civility of these little citizens more neatly sets forth the wisdom of their Maker. . . ." Yet, that we can design ethical robots, who may even invent games more fun than chess, is enough to prove that man's moral nature needs no supernatural source. Darwin observed, but Spencer failed to note, that success in the game of life, and so survival, is "often most promoted by mutual assistance."

Hence the crucial question: Can machines evolve? John von Neumann suggests that we are familiar only with simple machines that can make only simpler ones, so that we suppose this is a general law, whereas, in fact, complicated ones can make others still more complicated. Given a suitable Turing machine, coupled to a duplicator of tape and to an assembler of parts from a common store, it could make one like itself, put in a duplicate of its own tape, and cut loose its replica ready to make a new one like itself. There are now two. Their number will double with each generation. Variations compatible with this reproduction, regardless of their sources, will lead to evolution; for, though simpler mutations must fail, some more complicated will survive. Von Neumann, Wheeler and Quastler have computed the required complexity and find that, for general Turing machines to survive, they must be about as complex as a totipotent protein molecule, which is the simplest thing we know that does reproduce itself. Totipotent protein molecules are the littlest citizens. Man has not yet found their mechanical prescription. He has made amino acids by shaking together CO_2, NH_3 and H_2O in the light, and he has made polypeptides from amino acids. When he makes proteins by sticking these together he can better estimate the probability of their formation by chance in evolutionary epochs. If the civility of these little citizens only sets forth an evolved efficiency in forestalling the *Wärmetod*, we may forego the astronomers' cry against their Maker:

> What? From insensate nothing to evoke
> A sensate something to resent the yoke
> Of unpermitted pleasure under pain
> Of everlasting punishment if broke:
> Oh, were that justice and His holy right.

Following Wiener we estimate the complexity of a machine or an organism to be the number of yes-or-no decisions—we call them bits of information—necessary to specify its organization. This is the logarithm (base 2) of the reciprocal of the probability of that state and, hence, its negative entropy.

But Wiener has forerunners as well as followers in Cambridge. Charles Peirce first defined "information," his "third kind of quantity," as " 'the sum of synthetical propositions in which the symbol is subject to predicate,' antecedent or consequent." Of Peirce's friends, Holmes, in his *Mechanism in Mind and Morals*, excuses only volition from the sway of mechanical causation; and James, in several places, attributes the vagaries of the will to chance. Perhaps a New England conscience may afford freedom to its neighbors' wills, as Donne says we give "souls unto women only to make them capable of damnation." But surely he is damned already whose frame and fortune foredoom his failure. That he is the machine at fault insures that he and his neighbors hold him responsible. The common law construes intention from the deed, and a windmill that kills a man is deodand. Every psychiatrist who cares for the well-being of his patient comes to look on a man's sins as his misfortune of birth or breeding and is glad that his self-righteous brethren cannot climb into God's mercy seat.

Sin, in its widest sense, is but to miss a mark; and surely most of us are too familiar with self-guiding missiles to doubt that we can endow them with computers and target-seeking servos whereby to hit or miss their prey. The components of these circuits are too gross and inefficient for us to package in a head what fills the nose of a V-2 rocket. But given miniature efficient relays comparable to neurons, we could build machines as small to process information as fast and multifariously as a brain. The hardest thing to match is man's storage of bits of incidental information, but we can put

an upper bound on that. Following Craik's lead, man's acquisition of such information has been measured and never found to exceed a hundred bits per second of sustained reception. Were it 10 times more throughout his life, he could store no more than 10^{13} such bits. Heinz von Förster arrived at a similar figure by noting that the mean half-life of a trace in human memory is half a day, and the access to it over only 10^6 channels, with an access time of about 1 millisecond. Hence a man will come to equilibrium with far fewer traces than there are junctional buttons on our neurons. Moreover, von Förster showed that if, by regenerating traces, we retained some 5 percent of all our uptake, the energy required for this remembering would be only a fraction of 1 percent of that which flows through brains. This answers Bertrand Russell's only serious question about the peculiar causality of human thinking. Ashby, in his book, *Design for a Brain,* proposed a mechanism of adaptation that avoids the fallacy of simple location of a trace and makes the thing we are to seek in a given brain and its multiple locations depend upon the sequence of its learnings.

To the theoretical question, Can you design a machine to do whatever a brain can do? the answer is this: If you will specify in a finite and unambiguous way what you think a brain does do with information, then we can design a machine to do it. Pitts and I have proved this constructively. But can you say what you think brains do?

In 1953, in the symposium on consciousness of the Institute for the Unity of Science, Wilder Penfield used the term, as we do in forensic medicine, to mean precisely that his patient at a later date bore witness to what he also bore witness to as having happened then and there. Of course we can make machines do that. The questioner meant "Was the patient aware that it was he himself that did it?" which is self-consciousness, requiring but simple reflective circuitry. The physiologist would have settled the argument by defining 'consciousness' to mean 'responsiveness to present stimulation with a lag called latency'—a trait that few things lack!—but a psychoanalyst explained to me that "a patient is conscious of what he once felt only if at a later time he can verbalize it"—which is to say, "he is conscious of those things of which he says he is conscious"—and this requires only a machine that sometimes answers "yes" to this question. That is too easy; and if all we mean by consciousness is this ghost of half of mind-stuff, we may forget it all as just a pseudo-question. But I am sure that for every empirical scientist to whom existence is as primary as it is to a true Thomist what lurks behind this ghostly facade is the old Aristotelian "substance." To Helmholtz, it appeared as the *"locus observandi";* to Einstein as "the frame of reference of the observer"; to Russell as "the egocentric particular involved in denotation." For MacKay it yields the distinction between the languages of the observer and of the actor. Granted that we have objective knowledge of others, and substantial knowledge only of ourselves, this only proves us to be like every other thing, and divine, if you will, only as a part of all that exists. It does not demonstrate the metaphysical self-sufficient mind or soul with the unique property of perception. However one defines feeling, perception, consciousness, substantial knowledge—so the definition be finite and unambiguous—each and all are well within the tricky scope of circuitry. So much for the existential purport of machines!

Their second *raison d'etre* is to generate hypotheses. A mechanism that fits all our data is one of an infinite number of possible explanations of our findings. It always has properties disclosed by deduction and subject to the test of experience. It may even lead to an invention. Contemporary opinion, in Haldane's phrase, regards "every physical invention as a blasphemy and every biological invention as a perversion." This is less a matter of heresy than of "radical indecency." Plowing, milking, alcohol, coffee, tobacco, birth control, and artificial insemination are only the by-products of biological knowledge. The chromosome shuffling of Mendelian genes, which has stood the test longer than any other equally significant biological discovery, never offended our sensibilities, although it lets chance materially dictate our constitution. Only recently have we come to the data that set limits to the applicability of Mendel's law.

Each hypothesis predicts the outcome of numberless experiments. Hence, though no hypothesis can be proved, it may ultimately be disproved. A good one is so specific that it can be disproved easily. This requires a minimum of logical, or *a priori,* probability compatible with the data. I have sometimes boasted that my pet notion of the mechanism responsible for our seeing shape regardless of size was disproved by MacKay's experiment in my own laboratory. What grieves me is that neither I nor anyone else has so far imagined another specific mechanism to account for form-vision.

Perhaps in this "best of all possible worlds" neurophysiologists, like physicists, will be compelled to call their shots "on a cloth untrue, with a twisted cue and elliptical billiard balls." Russell has al-

ready noted that the explanation of mind has become more materialistic only as our matter has become less material. So we seem to be groping our way toward an indifferent monism. Everything we learn of organisms leads us to conclude not merely that they are analogous to machines but that they are machines. Man-made machines are not brains, but brains are a very ill-understood variety of computing machines. Cybernetics has helped to pull down the wall between the great world of physics and the ghetto of the mind.

Moreover, its analysis of nervous activity reveals two limits to our aspirations—our double ignorabimus. The impulses we receive from our receptors embody primary atomic propositions. Each impulse is an event. It happens only once. Consequently, these propositions are primary in the sense that each is true or else false, quite apart from the truth or falsity of any other. Were this not so, they would be redundant or, in the limit, as devoid of information as tautologies. But this means that the truth of each and every one cannot be tested. The empiricist, like the Thomist, must believe that God did not give him his senses in order to deceive him.

Moreover man, like his inventions, is subject to the second law of thermodynamics. Just as his body renders energy unavailable, so his brain corrupts the revelation of his senses. His output of information is but one part in a million of his input. He is a sink rather than a source of information. The creative flights of his imagination are but distortions of a fraction of his data.

Finally, as he has perforce learned from the inadequacies of his best hypotheses, ultimate universal truths are beyond his ken. To demand them is the arrogance of Adam; to come short of them is the impotence of sorry man; but to fancy them known were very ὕβρις. Obviously, he may know something about the past, although he cannot change it. The future he may affect, but he may never know it. Were this otherwise, he could beat the second law and build machines to operate on future information. So we may conclude that we fear no analogy between machines and organisms, either for existential purport or for generating hypotheses, and that we are safe to admit that organisms, even brains, are machines.

So long as we, like good empiricists, remember that it is an act of faith to believe our senses, that we corrupt but do not generate information, and that our most respectable hypotheses are but guesses open to refutation, so long may we "rest assured that God has not given us over to thraldom under that mistery of iniquity, of sinful man aspiring into the place of God."

Catoctin

Unseen
Her slow shrinking is anchored
In wild berries and shale.
Dipped to the eastward and west
She still lives
With maples near her heart and
Laurel for a crown.

—Edith G. Wies

The Transistor as an Industrial Research Episode

RALPH BOWN

Dr. Bown is vice president of Bell Telephone Laboratories in charge of long-range planning of programs and patent activities. He served as vice president in charge of research prior to his present appointment. Before that his work was concerned with various aspects of radio broadcasting, overseas telephony, and microwave radio relay systems. In World War II he was a division member and consultant of the National Defense Research Committee, specializing in radar, and he also served as consultant to the Secretary of War. Dr. Bown received his training at Cornell University.

IN order to describe the transistor as an industrial research episode (*1*), that is, how the transistor came about as a product of its time and environment, it seems appropriate to say something first about the transistor itself: to indicate what it is philosophically, that is, what its broad significance is; and to describe what it is physically, that is, what it is made of and, very briefly, how it works.

Since my purpose is to display and dissect the internal history and character of a particular laboratory episode, I have not attempted to include references to the admirable related work accomplished at other laboratories, both university and industrial. I have tried to indicate any lessons or other values that can be drawn from the experience.

The idea of a valve to control power is very old, certainly as old as the throttle of the steam engine or the sluice gate of the water wheel. The first equivalents in electric power were the switch and the rheostat. But all these things are slow-acting controls. It was the invention of the electronic valve, or audion, as it was first named in 1907 by its inventor, Lee De Forest, that made possible the lightning-fast, almost inertialess control of electricity which characterizes the present electronic age.

Forty years of exploitation of the flight of electrons in vacuum have brought such wonders, particularly in the communication arts, that we have not been very conscious of the limitations imposed by the power efficiency of electron tubes. Probably three-quarters of the hundreds of millions of electron tubes in use today operate with an over-all power efficiency of less than 1 percent, and I estimate that half of them are used in such ways that the efficiency, in terms of useful power out to total power consumed, is in the order of tenths, hundredths, or even thousandths of 1 percent. The lower the output level, the less the efficiency, because a certain minimum of power, about 1 watt in most cases, must be supplied to an electron tube to keep its cathode hot—in other words, to keep it in operation at all.

The transistor is an electronic valve that can operate efficiently at any power level from a few watts down to about a millionth of a watt. Potentially it can do many kinds of jobs, with efficiencies ranging up to 50 percent, that the electron tubes of today are doing inefficiently or cannot do at all. All the other features of the transistor (Fig. 1), its minute size, its ruggedness, and its potential inexpensiveness, become relatively insignficant beside the overwhelming fact that it is an efficient low-level electronic amplifier—a basically new thing in the world.

But this does not mean that electron tubes are becoming obsolete. In many cases power efficiency is not important. Furthermore, electron tubes are also used to produce outputs up to many kilowatts. In this range they can be efficient, and in this range the transistor cannot as yet operate. Likewise, electron tubes can handle alternating currents at microwave frequencies, frequencies that transistors have not yet achieved.

The external appearance of a transistor depends on the kind of encapsulation used. Several transistors are shown in Fig. 2. How are they made and how do they work? The two most common forms, known as the point-contact transistor and the junction transistor, are different in construction but not so different in theory of operation. The point-contact transistor (Fig. 3) is a small piece of germanium crystal held in a mounting that also supports two fine-pointed wires with their tips bearing against the crystal surface only a few thousandths

of an inch apart. A junction transistor (Fig. 4) is a tiny bar of germanium crystal composed of germanium containing, say, negative electron carriers. The bar is divided into two parts by a thin curtain extending completely through it. In this barrier region electric carriers of opposite sign—that is to say, positive carriers—predominate. It is a sort of sandwich, and it is designated as an n-p-n junction transistor to indicate, by the letters n and p, the negative and positive nature of the electric carriers in the different parts; p-n-p transistors also are made.

The transistor thus has three electric parts that form its three electric terminals. In the point-contact transistor the piece of germanium is called the base, and the two wires are the emitter and the collector, respectively, depending on how they are treated and connected. In the junction variety the end sections are the emitter and the collector, and the central section is called the base. Transistors have been made of other semiconductors, but at present germanium is most widely used.

Pure germanium has the same crystal structure as diamond, each atom having four symmetrical valence bonds (Fig. 5). In its pure crystalline form germanium is a relatively poor conductor of electricity. If an atom of arsenic having five valence bonds gets into a germanium crystal it can substitute for a germanium atom. except that it has an extra valence electron. This electron can move through the crystal lattice under the influence of any voltage that may be applied, thereby giving the crystal greater conductivity. On the other hand, if an atom of gallium is introduced, it, having only three valence bonds, leaves a hole in the structure where an electron is missing. When an electron from another position jumps into the hole, the hole

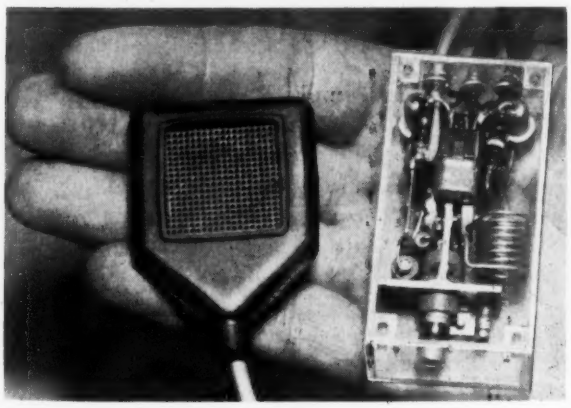

Fig. 1. A complete frequency-modulated radio transmitter employing two transistors. The microphone is almost as large as the radio transmitter.

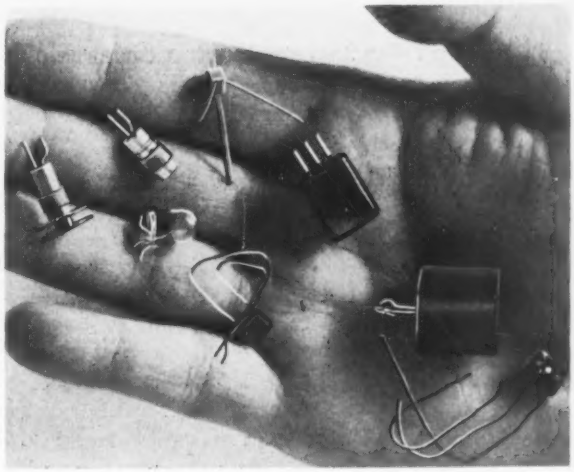

Fig. 2. Transistors are built in a number of forms for various specific purposes.

in effect reappears at or moves to the position from which the electron jumped. Thus the hole acts as a positive carrier and can move about under the influence of an applied voltage like an electron but in the opposite direction. Physicists refer to the carriers as holes and electrons and characterize impurities in germanium as donor or acceptor materials, depending on whether their atoms contribute electrons or holes to the basic crystal structure. In the germanium environment the carriers are already in being and ready to respond to electric fields; no heat is required to make them available, as in the electron tube. That is why the transistor can be efficient at even the lowest power levels.

I do not propose to explain just how these carriers interact in the different parts of a transistor. The essential result is this: if an input current is set up between the emitter terminal and the base terminal (Fig. 6), and if a load circuit and a suitable source of power, such as a battery, are connected between the collector terminal and the base terminal, the ensuing current which the transistor permits the battery to send through the load will be an enhanced replica of the input current and able to deliver much higher power. The source of the power is the battery. The control agent is the input current. Thus in the transistor we have an electronic amplifier, or, as the British logically call it, an electronic valve. This little piece of germanium crystal with three wires attached to it is ready to do business on demand, efficiently, and down to minute energy levels.

What is the history of this simple but potentially universal servant of man? The use of semiconductors in communications began at least as far back

as the crystal detectors employed in early radio receivers before the electron tube was known. The fact that semiconductor materials change their resistance radically with temperature, and with the direction and size of an applied voltage, was made use of in ensuing years for applications ranging from lightning arrestors on electric power lines to regulating, measuring, and modulating devices in wire telephone systems.

It was the electric versatility of these materials that led M. J. Kelly, then director of research at Bell Telephone Laboratories, and his associates to launch, immediately after the war, a renewed investigation of the properties of semiconductors as one branch of a program of fundamental research into the physics of solids. The purpose of the semiconductor program, as it was envisioned by J. B. Fisk and W. Shockley, who had direct responsibility for the initial plan, was to gain a deeper understanding of the physics of these substances.

As the work of the research group progressed, and an improved theory explaining experimental facts began to take shape, these men started also to speculate on ways in which the old dream of a control valve for electrons flowing in solids might be realized. Shockley, who was the leader and also a working member of the group, evolved a proposal wherein an electrode, external to a piece of semiconductor, would be expected to influence the flow of the carriers inside the solid material. An experiment was devised to test the scheme, but the results were disappointing at first. There seemed to be something going on at the surface that prevented penetration. At this point John Bardeen suggested a theory of surface states to explain the experiments. In the course of further experiments to test the theory, carried out in collaboration with Walter Brattain, these two investigators jointly invented the point-contact transistor (Fig. 7).

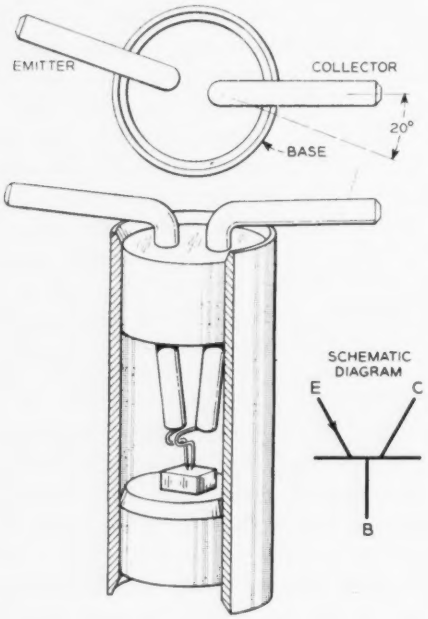

Fig. 3. Cutaway view of a point-contact transistor.

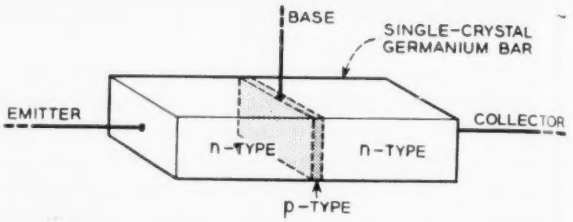

Fig. 4. The heart of a junction transistor is a tiny bar of germanium having three layers to which three electric connections are made.

In this effort we had the traditional motivation and method of scientific research intermingled with the motivation and method of the inventor. The scientist seeks to know and to explain; the inventor is alert to apply. In combination they can create promptly and soundly the new things that their work has made possible. The purity and integrity of science are in no sense stultified by the immediacy with which it is translated to human service.

I can well remember the day on which Bardeen and Brattain got their first definitive results. I was at that time director of research, having taken over from Kelly a year or two before. I was called out to the laboratory to look at the movements of the needles of a couple of electric instruments responding to manipulation of an applied electric stimulus in the setup. The action was crude and sluggish, but its reality was not doubted. During ensuing days of experiment and study the discovery was evolved into a device that amplified speech.

The acid test of an amplifier is whether it can be made to oscillate. If it really can deliver more controlled power than it takes to exercise the control, a connection from output back to input makes it unstable and results in a continuously oscillating system. They had not yet made it oscillate, but the very next day they did have it oscillating, and we were all completely satisfied that the thing in hand was real and important.

What does one do when one has such a technologic bonanza to deal with? One knows that even if one wants to keep it secret, news such as this can be contained only a short time. It was decided to

follow our traditional policy of promptly publishing the work after a decent interval had ensued for verification and for filing initial patent applications. Meanwhile the most gifted electronic engineers from all parts of our laboratory were brought into a concerted plan to study the device from all angles and to learn how to use it to do the various things an amplifier should be able to do. Experience with electron tubes and their uses was merely of suggestive value, because this device was so completely different that it required a new start in many respects.

Bringing in everyone in the laboratory who might be expected to have a good suggestion or related idea, and giving them encouragement to work on it, was the first move. The second was to set up a development group completely separate from the research group. Study of ways to employ the transistor could not proceed without a supply of samples with characteristics satisfactory for various functions. If the research group undertook such development their research would come to a halt and further fundamental advances, and perhaps new related inventions, would be delayed. Drastic moves such as these, taken in a short time, can be made only if a laboratory program is mobile and its leaders have real freedom of decision and action.

In planning the first public announcement of the transistor we decided again to adhere to our traditional policy of talking about things that had actually been accomplished rather than about hopes and aspirations. This led to a failure on the part of the newspapers and newsmagazines to appreciate

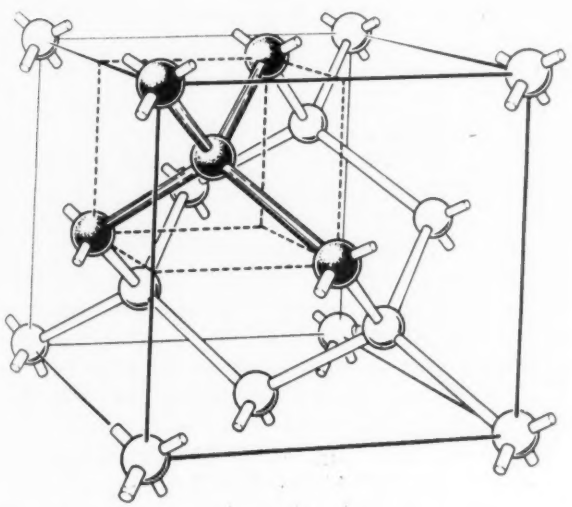

Fig. 5. Germanium has the same crystal structure as diamond.

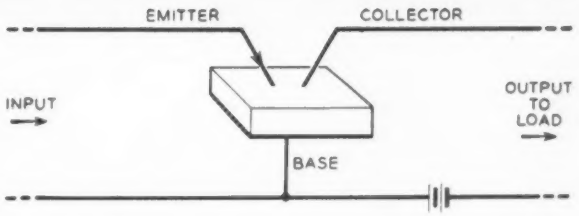

Fig. 6. A simple transistor amplifier circuit.

the significance of the announcement but it insured that the electronics industry would have immediate and proved information. A press demonstration was conducted on 30 June 1948, in which transistors were shown working as speech amplifiers, as oscillators, as pulse amplifiers, and as television amplifiers. In addition a radio broadcast receiver that did not contain a single electron tube but only semiconductor rectifiers and transistors was shown picking up and reproducing programs at room volume. A little while later this demonstration was repeated for about 300 executives and engineers of the electronics industry. The announcement was timed to coincide with the appearance of the first scientific papers describing the technical features of the transistor, so that it would be possible for others to test the results and to make their own experiments.

Two aspects of our policy are illustrated by the procedure I have just described. We hold that a laboratory that draws knowledge and trained people from the world of science is thereby under some obligation to return value in kind. By prompt publication of its scientific advances a laboratory makes this return to the common fund. I think that prompt scientific publication is a sound policy for any research enterprise to follow as much as it can. This policy, furthermore, is not based wholly on a sense of moral duty but contains a modicum of self-interest. Science and technology advance through competition and the interplay of bright minds. No one laboratory has more than a fraction of the talent that can operate usefully on a new scientific idea. Exposing the idea to other minds will lead to more advance. And who is in a better position than the originator himself to recognize and profit from further advances?

In the next step of our story the transistor gets into trouble. Making a few laboratory point-contact transistors to prove feasibility was not difficult. But learning how to make them by the hundreds or thousands, and of sufficient uniformity to be interchangeable and reliable, was another problem. Experimenters despaired of getting consistent results with the sample transistors, and potential users lost

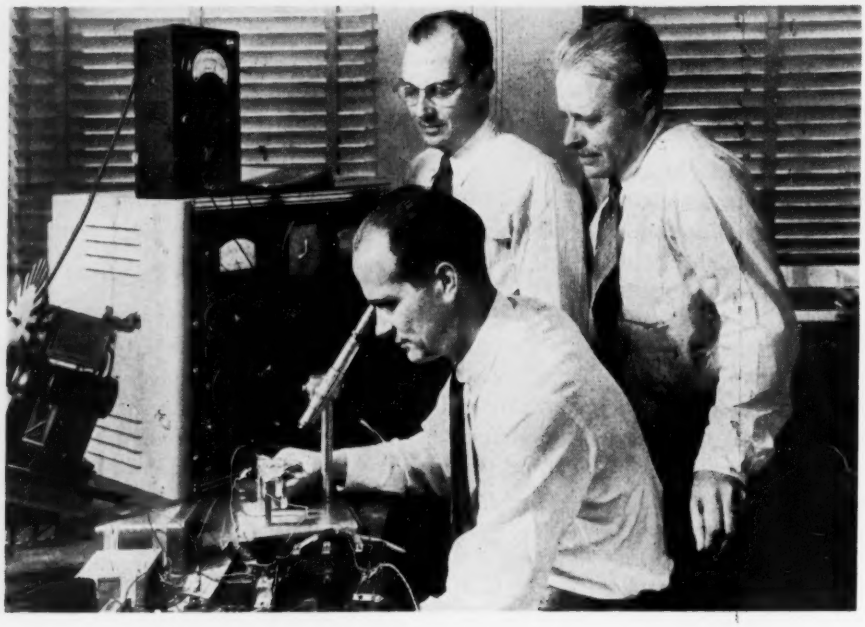

Fig. 7. The physical principle on which the transistor functions was discovered and explained by means of many theoretical studies and extensive laboratory investigations. William Shockley, (seated), together with the co-inventors of the transistor, John Bardeen and Walter H. Brattain, are shown in the laboratory considering the crystal structure of semiconductive materials such as germanium.

interest. But the work continued to bore into fundamentals, and finally we were able to prove that some of the variables were introduced by flaws and grain boundaries in the polycrystalline germanium ingots being cut up into transistor blanks.

In their desire to get at the properties of germanium in its ideal state, the research group had learned how to grow the material in large, single crystals. Blanks cut from large, single crystals gave more uniform results. Strides made in the elimination and control of impurities also gave a key to quality control, and a couple of years of intensive fundamental research and development effort supplied the knowledge whereby pilot-line manufacture could produce a uniform product.

The research study had by now advanced to the point where a 500-page book was needed to cover adequately the physics of semiconductor and transistor theory. The classic volume *Electrons and Holes in Semiconductors,* by William Shockley, was published in the Bell Laboratories Series (*2*) in 1950. In one chapter of this book Shockley delineated and analyzed a device he called the junction transistor. The basic theory of semiconduction already had been so perfected that this device could be conceived and invented and its properties predicted—all on theoretical grounds. The junction transistor was difficult to construct, however, and it was some time later that a process was worked out of dynamically modifying the donor and acceptor content of the melted germanium, from which a crystal is grown, so that junction transistors could be cut directly from the layered crystals thus produced. The new transistors proved to have properties even more favorable than had been anticipated. Not only was their efficiency excellent at extremely low power levels, but the noise generation that had plagued the point-contact transistor was much reduced. It was evident at once that here was transistor performance of a new order, having almost limitless possibilities, including military applications.

About this time the cold war had warmed to a hot one in Korea and the question of security intruded itself. In the original transistor announcement, in 1948, the military services were given a preview a week before the press and thus were offered opportunity to comment on the publication policy if they wished to do so. Now again the matter was brought to the attention of the three services, and our policy of free publication of the science and use of transistors was not affected. The process of manufacturing transistors is commercial information that has been disseminated to those having legitimate reasons for access. Patent licenses have been made available on reasonable terms, and some 40 companies have been licensed to make and sell transistors as such. More than 500 companies are licensed to make, use, and sell them as part of equipment that they manufacture. Royalty-free licenses have been given to all who desire them for hearing-aid manufacture.

About the only functional things that present transistors are not able to do which might be desired of them are to handle high power and to work at extremely high frequencies. In the realm of

high power, the electron tube is a satisfactory and efficient device; there is no real need for the transistor. But in the high-frequency region of communication technology the electron tube suffers all the disadvantages of inefficiency that it has at lower frequencies.

One of our engineers has already invented a type of junction transistor, called a tetrode transistor, that amplifies efficiently at the highest frequencies employed in wire and coaxial cable transmision of telephony and television. Also, it seems likely to be able, if need be, to take over all but the highest frequency parts of microwave radio relay amplifying functions. Its possibilities in radio and television receivers are potentially great.

There is no theoretical reason for believing that transistors cannot be devised to work at frequencies as high as any other electronic device. Doubtless one of the electronic laboratories now working in this field will solve the problem in the not too distant future.

There are some things about the progress of this transistor research and development that may be of particular interest to those who themselves have the problem of integrating the use of different sciences and different scientific skills toward a common end. The initial research group on semiconductors included experimental and theoretical physicists, a physical chemist, and an electronic circuit engineer. It was substantially a self-sufficient unit capable even of providing much of its own special instrumentation. This kind of a group makes for flexibility and mobility of thought and action.

Once the transistor had appeared and a larger volume of effort was justified, two paths were available. We could increase the size of the original group, letting it grow as much as necessary to encompass the whole activity. Or, alternatively, we could spread the activity through our essentially functional organization so that the groups of specialists in all parts of the organization would be tapped. It was the latter course that was followed, and I think with profit.

In our laboratory we have traditionally kept our physicists, our mathematicians, and our chemists in well-knit groups where they would associate with one another in communities of their own special interest. Such a grouping plan tends to create dividing walls of thought, and alongside such walls often are moats in which good ideas may sink out of sight. To combat this we have had frequent interchanges of personnel, particularly between the areas of physics and chemistry, and particularly at the supervisory levels. Also, we have not only a department of physical research and one of chemical and metallurgical research, but a department of chemical physics that includes the physical instrumentation now so widely used in chemical analysis and the special chemistry of electronic device materials.

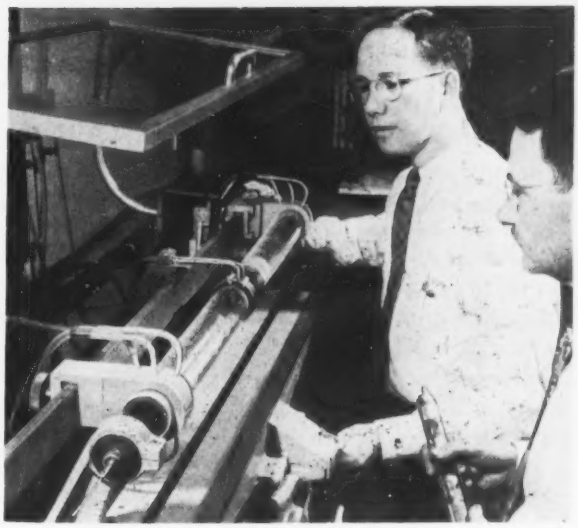

Fig. 8. A new metallurgic technique, the "zone-refining" method, for producing extremely pure germanium being discussed by J. H. Scaff (right) and its inventor W. G. Pfann.

Having each specialist group undertake some work on transistors, emphasizing the interests and ideas inherent in its background, led to wider and more rapid progress because the decision in choice of work was also widely scattered. Things were done, for example, to bring about remarkable advances in metallurgic processes of preparing germanium (Fig. 8) that were not to be expected of a project-type group; an independently working chemist, following his own instincts, identified minute copper contamination as the cause of a disturbing phenomenon that had not yet yielded to the physicists' attack.

For the shakedown period, until the transistor work had taken its permanent form in our organization, technical coordination was handled by a three-man committee composed of a physicist, a chemist, and an engineer, each a key supervisor. Also, we held, periodically, internal meetings, patterned after scientific society meetings, at which papers on transistor work were presented to keep all interested laboratory members well informed of progress in other departments.

To sum up this experience, I think I can say that we feel surer than ever that our method of organizing our laboratory into a primarily functional structure of specialist groups is a good plan, provided that there is flexibility for dealing with unusual situations as they arise.

One might well question how, in an apparently loosely hung activity such as this transistor program, with its great opportunities for valuable inventions, one can avoid destructive rivalries. Scientists and engineers are human beings, and human beings are by nature competitive. I believe that the destructive factors in competition are greatly mitigated by two of our policies. The policy of easy individual publication insures that in each scientific advance the scientific world will know who has contributed what. The policy of making no special awards for inventions but of considering an invention as one meritorious performance to be lumped with all others in the determination of salaries reduces the incentive that leads to dog-in-the-manger thinking.

What is the general status of transistors today? The answer to this question can be formed by flipping through the pages of a current magazine, particularly a technical magazine, and noting the advertisements. In the public eye the transistor already has arrived. But is this truly the case? Certainly there are many puzzling technical and economic problems yet to be solved. What has been going on is a phenomenon that could occur only here and in our time. Industry has spent many millions of dollars during the past 3 or 4 years on research, development, and pilot manufacture of a new and almost untried device. The actual operating usage to date is almost negligible. This phenomenal build-up has occurred because the research scientists have understood the new thing with which they are dealing. The development and manufacturing engineers could meet their problems with the confidence of accurate knowledge. And the businessmen have had faith that the scientists and engineers really know what they are talking about. It seems to me that this is the combination it takes to accomplish the true mission of industrial research.

References and Notes

1. Based on a paper presented at the fall meeting of the Industrial Research Institute, Inc., Detroit, Mich., 26–28 Oct. 1953.
2. W. Shockley, *Electrons and Holes in Semiconductors*. Bell Laboratories Ser. (Van Nostrand, New York, 1950).
3. A new high-frequency transistor has recently been announced. J. M. Early, "P-N-I-P and N-P-I-N junction transistor triodes." *Bell System Tech. J.* **33**, 517 (1954).

Besides ministering to our comfort, science also serves to satisfy certain needs of the human spirit. It helps us to understand the world and to feel at home in it. We are distressed by disorder, and always try to arrange in order the things with which we have to deal, whether they are the affairs of a nation, the books in the library, or our own ideas. Science satisfies us because it shows us that, behind the transient and confused pageant of nature, there is a permanent and orderly reality. And it was in the sky that this order was first revealed to men on a vast and spectacular scale.—A. ARMITAGE, Sun Stand Thou Still (*H. Schuman, New York, 1946*).

Facts, Thoughts, and Dreams

J. A. GENGERELLI

Dr. Gengerelli is chairman of the department of psychology at the University of California at Los Angeles, where he has been since 1929. He received his training at Ohio University and the universities of Wisconsin and Pennsylvania. During recent years his research has been divided between physiological psychology as it relates to learning phenomena, and psychometrics. This article is based on Dr. Gengerelli's address, as retiring president, to the U.C.L.A. chapter of the Sigma Xi, 2 June 1954.

THERE is a school of scientists, or rather it would be better to say, there are many scientists (and some of them of considerable importance) who take a rather pedestrian view of the scientific enterprise. I find myself likening them to librarians whose chief preoccupation is acquiring books and seeing to it that they are classified in some manner or other. Many naturalists of the 18th and early 19th centuries exemplify this style of science most clearly; in the great euphoria that followed its unshackling from the constraints of authority and dogma, the human mind took off in the direction of untrammeled empiricism—the wholesale gathering of "facts" and "statistics." A quotation from O. Henry irreverently, albeit vividly, illustrates what I have in mind:

> "Let us sit on this log at the roadside," says I "and forget the inhumanity and ribaldry of the poets. It is in the glorious columns of ascertained facts and legalized measures that beauty is to be found. In this very log we sit upon, Mrs. Sampson," says I, "is statistics more wonderful than any poem. The rings show it was sixty years old. At a depth of two thousand feet it would become coal in three thousand years. The deepest coal mine in the world is at Killingworth, near Newcastle. A box four feet long, three feet wide, and two feet eight inches deep will hold one ton of coal. If an artery is cut, compress it above the wound. A man's leg contains thirty bones. The Tower of London was burned in 1841."
>
> "Go on, Mr. Pratt," says Mrs. Sampson. "Them ideas is so original and soothing. I think statistics are just as lovely as they can be."

This excerpt is not intended to belittle even in the slightest degree the unquestioned importance of empirical fact for science. Science is bound irrevocably to experience; empiricism and perception are its touchstones and the secret of its greatness and incorruptibility. In science, "faking results" is rare, not because the ethics of scientists are superior to those of other mortals, but because science deals in perceptions, not in beliefs or creeds; hence as long as there exists a pair of eyes in the world, the "fake finding" is liable to check. The point is not that fact-finding should be given, so to speak, a secondary role in the scientific enterprise; rather, it is that there are so many facts that may be ascertained. Consider, for example, the blades of grass on a lawn; what a capital fact-finding project they would make! We could use the very best in stratified sampling devices to calculate an unbiased estimate of the number of blades of grass contained in the defined area. Pursuing the matter further, we could make certain covariational studies, such as ascertaining the mathematical function that relates this number to the time, amount of water used, and mean daily temperature.

The lure of fact-finding is augmented by the beautiful equipment that now exists for making observations of the greatest precision: fantastically sensitive meters, balances, and gages, to say nothing of microsecond chronometers, are almost a commonplace in many laboratories. What an opportunity they provide for making elaborate and careful investigations involving a maximum of diligence and scientific control and a minimum of thought!

The love of facts for their own sake that characterizes many scientists finds ready critics in numerous quarters. We are a nation of pioneers, not very far removed from the frontier, and we are full of "projects." The critics are quick to challenge the value of facts *an sich*: there are too many of them, and of what *use* are they? If you must investigate facts, there should be a principle of selection, and the principle enjoined by the so-called "man of affairs" is that of practicality: those facts should be studied that lend themselves to the furtherance of some project. And so the pedestrian fact-finder in science is saved from himself by the important principle of practicality. The solution is an especially happy one, for he is financed hand-

somely to make ever more precise observations with ever more elaborate instruments that require ever more time and technical assistance to be kept in functioning order. As long as the financing continues we have a social force that selectively encourages and rewards the scientific hack. He is the man who becomes enormously competent in an ever smaller field, who has read the latest paper on the subject—or, better still—whose assistant has abstracted it for him. He is the man who concentrates his energy and physical resources on refinements of existing mathematical procedures and physical instruments, with the consequence that the latter become so elaborate and intricate that they can be procured only by the *very best* laboratories.

With not a little diffidence—one man's knowledge can hardly blanket so much territory—I wish to express the gradually formulated belief that, judged from a statistical point of view, American science tends to be characterized by the technologic rather than by the formal and theoretical. Perhaps this is a point that has been frequently made; perhaps not, although I seem to remember encountering some such attitude as this somewhere in my readings. At any rate, at least a superficial survey of mathematics and the empirical sciences yields a predominance of European names. The great scientific creators and theoreticians of modern times bear such names as Maxwell, Hamilton, Einstein, Planck, Heisenberg, Lobachevsky, Kantor, Berger, Claude Bernard, Sherrington, Freud. When I try to think of a great American theoretician and creator in pure science, I conjure up the name of Willard Gibbs; then I find I am stuck, and no other name comes to mind. There can be no doubt that this predominance of Europeans in the domain of pure scientific creativity, even if we consider only the time segment since the latter part of the 19th century, is the result of what might be called the scientific momentum in European culture. In other words, many educated Europeans had been studying pure science and had a tradition of interest in it centuries before our Republic was born. As time goes on, the difference, if it truly may be said to exist, will be eliminated.

The major objective of this paper is precisely the reduction of this difference in the theoretical and abstract domain. We have long since stopped fighting Indians; we have had at our disposal for at least one generation all the physical facilities that are available in our time; indeed, we lead the world in scientific facilities. There is therefore no longer any excuse for our not producing scientists of the highest order of creativity and imagination. It is customary these days to pay obeisance to research and even to fundamental research. It has almost become a fad; even those who are least interested and talented in original creative work speak briskly about it, as if it were something that we should do at regular intervals, like going to church services, and being a good Christian on Sunday.

Now, as everyone knows, there is a great deal of research money floating about in this country. The Congress has created the National Science Foundation to encourage training in, and the creation of, fundamental research. Nevertheless, the suspicion remains that the very strength of our motivation in this direction and the opulence with which it is implemented are in themselves obstacles to successful realization. Even behind the numerous exhortations to "do fundamental research," one often thinks he hears the unspoken thought: "Hurry, so we can get some new fundamental knowledge to put to practical uses." There is, consequently, or so it seems to me, a great hurry and bustle, a rushing back and forth to scientific conferences, a great plethora of $50,000 grants for $100 ideas. The thing to do is cook up a plausible, well-rounded project, garnish with a fine-sounding programmatic introduction, and seek a generous budget that utilizes the most elaborate equipment available as well as a goodly number of graduate student research assistants. Perhaps I am becoming somewhat bilious with advancing age, but I catch myself noting that the stature of a scientist is measured by the size of his research grants. Obviously there is nothing wrong with an investigation just because it is expensive and elaborate; what I am trying to indicate is that, with all this money available and crying to be spent, there is developing the attitude that research is a business like any other. You begin at 9 A.M., you take off an hour for lunch —or a little more if you can arrange a plausible conference—and knock off at 5 P.M. Between then and the next morning you exclude thoughts of science or scientific ideas; it might make you stale for the real work of the day.

Again, in justice to myself, I must say that I would be one of the last to underestimate the tremendous importance of the pedestrian day-to-day aspect of scientific work—the eternal calculating, the dishwashing, the wiring and soldering. I am merely trying to say that we seem to be slipping into a national habit of putting our best efforts into making requests for research grants, administering them, and, above all, thinking up reasons for a renewed and much expanded grant next

year. The thinking, the silent brooding, the work of the imagination, the probing and going back to first principles, are relegated to a minor role and are pushed into the background. How often do we not finish reading the report of an elaborate, carefully controlled, time-consuming and expensive experiment with the half-formulated thought: "If the experimenter had experimented less and thought more, perhaps something significant would have happened here." We have, as I said earlier, such beautiful gadgets, so marvelously precise, and such monstrously clever mathematical and statistical devices that even the most modest talent can make motions like a genuine creative scientist. I am suggesting, in fact, that scientific, technical, and financial facilities are such in this country as to encourage a great number of mediocrities to go into science and to seduce even those with creative talent and imagination to a mistaken view of the nature of the scientific enterprise.

Therefore I wish to plead the cause of imagination and reflective thought in American science. To dream, to brood, to go back always to first principles, to live with a problem not only during laboratory hours, but at odd times, these are the necessary, though naturally not sufficient, conditions for achievement at the highest levels of pure science. These are characteristics of great artists, poets, and musicians as well as of great scientists—an observation that has, of course, been made many times. We must imbue students of science with this style of life, so to speak, and impart to them the feeling that it is the *idea*, not the gadget that has the priority in science. True, it happens often enough that an idea cannot be tested unless a suitable gadget is at hand, but, then, if imagination and ideas abound it is easy enough to make gadgets. The young scientist should grow up to look upon himself, not as a *doer*, but as a *solver* of problems, as a mind whose imagination and incisive analytic power is such as to bring an ever larger domain of established fact under ever fewer and simpler scitific principles. The careful experimental plan and the cunning execution are at best only coequal to pure thought in this grand design, and not formally prior.

Imagination and theory-building yield their grandest triumphs when they batten on a rich and wide store of information. The scientific man who would contribute significantly to a particular field in science must be familiar with fields other than the field that he wishes to make his own. As a matter of fact, there is such a thing as knowing too much about one's own field—so many of the things that are "known" are not likely to be so, or, at best, are badly misinterpreted. All this too-detailed knowledge only serves to inhibit the investigator and keep him in a rut. It is a grim fact often repeated in the history of science that the big break-through in a given field was achieved by a theory, an insight, in short, a development in another field. Witness, for example, the influence of quantum theory on the development of chemistry; the effect of the mathematics of groups on quantum mechanics; the relationship of the restricted theory of relativity to nuclear physics. These facts are bound to give pause to the scientist who never looks beyond the limits of his own vineyard. Indeed, one may boldly say that a fair guarantee of increasing erudition and scientific sterility may be obtained by pursuing a narrow area of investigation, reading, and thought during the whole of one's professional life.

The student of science should, especially in his earlier years, read, think, and study widely in the various disciplines. This means that he will write fewer papers at first, but it will pay off handsomely in the long pull, in terms of increased power and extent of intellectual resources, scope of imagination, and play and fertility of ideas. It is a mistake to stoke up on all the impedimenta of a specific field, all the experiments, all the literature, and all the folklore too soon; he who becomes an "expert" too early in his scientific life is likely to look increasingly "corny" as the years go by. Such a person does not have the breadth of knowledge to give scope and spring to the imagination. Thus we have the picture of a man who has done too much and thought and dreamed too little. From the point of view of American science as a whole, the worst of it is that persons like this are likely to be chairmen of the most important national committees, for they have been famous experts for such a long time!

And so my argument has made full circle. My insights, such as they are, bespeak the importance of imagination, vision, and audacity in science and the necessity for wide knowledge and catholicity of interest in several scientific domains, to give imagination the necessary substance and audacity the necessary fulcrum. Imagine more, reflect more, think more, even at the cost of experimenting less. With these vague reminiscences and free associations, I come to the end of my story.

Validity of Test Items That Involve Finding a Pattern in Data*

RICHARD H. LAMPKIN

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THE ability to find patterns in data seems necessary for scientific research, in which hypotheses, theories, and laws are formulated in order to summarize past experience and to predict future experience. Such ability seems to be an aspect of general mental ability.

Situations in which it seems desirable to measure this special ability include testing intelligence, evaluating instruction, admitting to college, guidance counseling, and hiring research personnel. One kind of test item that has been devised for this purpose uses number series. A number series is defined here as a group of numbers in which numerical size is related to order of presentation. Twenty-six tests are cited (*1–19*) which have 409 such test items, either grouped in subtests or mingled with other items. A specimen item follows, with likely keyed answers inserted in the spaces provided. There is no essential difference for our discussion if multiple responses are suggested and a standard I.B.M. answer sheet is used.

Specimen test item that uses a number series

Instructions: The numbers in each problem are arranged according to some particular scheme. In the spaces provided, write the next two numbers in the series.

Item					Answers	
0	2	4	6	8	10	12

The discussion here is confined to items that use number series, but the reader should be able to extend the remarks to closely related items that use letter series (*20*) and number sets (*21, 22*), and to items that report alleged experimental data (simultaneous values of two or more variables) from a different universe and ask the subject to derive an experimental law representing the data (*23, 24*).

Some of my remarks may well apply to still other varieties of items used in testing ability to find patterns in data. Such items employ sets of abstract geometric figures, sets of words, and sets of things represented by realistic pictures or by words; the instructions are to find the one that does not belong with the others or to choose from several suggested responses another that does belong. However, I shall not discuss the issues here or cite any specific sources.

It seems to me that test items that use number series to measure ability to find patterns in data have several defects. It is the purpose of this paper to call attention to these defects and to suggest that such test items seem not to be valid and perhaps should not be used any more.

Plural solutions are always possible. The specimen test item seems simple, at first glance. Counting by twos may have been the "particular scheme" that the test maker had in mind and I will call it scheme A. In mathematical symbolism, it is $x_n = 2n - 2$, where x_n is the numerical value of the term whose serial number is n. According to scheme A, then, the answers are 10 and 12.

But thinking of the series as counting by twos need not keep us from thinking of it in other ways. We may think of it as proceeding according to scheme B, namely: x_n is 0, if dividing n by 8 leaves a remainder of 1; is 2, if the remainder is 2 or 0; is 4, if it is 3 or 7; is 6, if it is 4 or 6; and is 8, if it is 5. According to scheme B, the answers are 6 and 4.

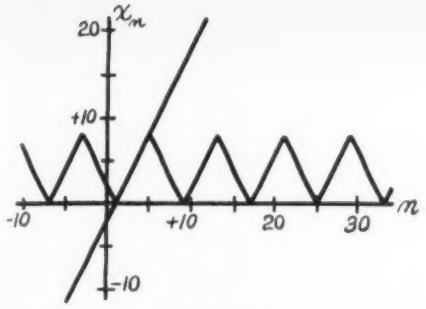

Fig. 1. Schemes A and B. Scheme A, which appears in each figure for comparison, is $x_n = 2n - 2$. Scheme B is explained in the text. In all figures, the several schemes have been extended into negative values of n.

In Fig. 1, scheme A is represented by the diagonal line from lower left to upper right, while scheme B is represented by the saw-tooth line. The diagonal and saw-tooth lines run together for a short distance, each representing the given terms in the number series. But they run differently on each side of their common segment, representing the distinctive features of the two schemes. In each of the other figures, the diagonal line occurs along with another curve representing still a different scheme.

For example, in Fig. 2, scheme C is represented by a different kind of saw-tooth line, which nevertheless coincides with the same short segment of the diagonal line. Scheme C in words is that x_n is the n successive remainders in the quotient $11(n-1)/9$ as n varies from 1 to infinity. Scheme C yields 1 and 3 as answers to the specimen test item.

Scheme D might be that some operation described as chance has produced the numbers, or that it is not possible to tell from any term what the next term will be. For example, the digits might have been selected from an extensive table of random numbers, such as Tippett (*25*). Or the numbers might have been obtained by opening a valve to let flow a stream of pseudo-random numbers derived from the use of Lehmer's rule and a calculating machine (*26*). Clearly the method of chance would provide a large number of solutions, and no figure is given for scheme D.

The first four schemes consider the numbers in the series to be pure and exact. However, in view of the assumed similarity between the test items and the practice of scientific research, we may consider the numbers to be like experimental data in that they have been stated to an appropriate number of significant figures. On this assumption, the possible schemes include in addition those represented by plural infinities of curves through the plotted points which deviate from the points by as little as may be desired.

If we wish, we may limit ourselves to schemes suggested by natural phenomena, for example, schemes E and F (Figs. 3 and 4). Or we may consider any kind of functional relationship. For example, let us draw a circle of, say, 20,000 radius tangent to the line of scheme A at the point (3, 4). Arcs of the circle would not differ appreciably from the line of scheme A for n ranging from 1 to 10 or thereabouts. An infinity of such circles could be drawn to the left of the line with larger and larger radii; a similar infinity could be drawn to the right. Then we could draw ellipses—infinities of them—and infinities of parabolas, infinities of hyperbolas, infinities. . . .

We might suggest a few interesting curves of other types. They are sketched in Figs. 5–9, and their equations are given. One who is skilled in analyzing equations and tracing curves will quickly see that for small values of n each of the equations of schemes G through K becomes approximately $x_n = 2n - 2$, the linear equation of scheme A. The curve of each passes near to all the points of scheme A for small values of n, but they each diverge from the diagonal line, in different directions, more or less rapidly, some never to meet the line again, one to cross it only once again after an excursion (scheme J), and another to cross it again and again after repeated excursions to positive and negative infinities (scheme G).

These schemes show that it is possible to find more than one "particular scheme" consistent with this series and to give answers that are defensible but different from the keyed answers. I believe that I have demonstrated more than particular alternative schemes and answers for the specimen test item. I have demonstrated by example large classes of alternative schemes that can be applied to any such test item. Schemes B and C represent the incorporation of data into a larger pattern, or longer period phenomenon. Scheme D is the operation of chance. And plural infinities of curves, some sug-

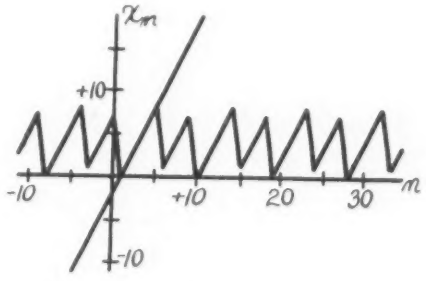

Fig. 2. Schemes A and C. Scheme C is x_n equals the n successive remainders in $11(n-1)/9$ as n varies from 1 to infinity.

gested by natural phenomena, some such as conic sections, trigonometric functions, or any suitable theoretical relationships can be sketched through the points. Since plural solutions can generally be found, items of this kind have been shown not to be valid so long as the key accepts only one.

Patterns seem projected rather than inherent. Let us consider why more than one scheme is possible, although the test instructions assure the subject that the "numbers . . . are arranged according to some particular scheme." The task set is to discover an inherent scheme. The setting of such a task may be regarded as based on the philosophic view that experience is explainable in terms of a master plan that our scientific investigations may reveal more or less.

A contrary view is that we project our own schemes into the data, as when we look at the ink blots of the Rorschach test and organize them in our own way, or when we read textual material that the author has doubtless intended to be interpreted uniquely and read our own ideas into it (27). In this view, the item might be improved philosophically if the assurance that the "numbers . . . are arranged according to some particular scheme" were deleted. Perhaps it would be better to say, "See how many patterns you can project into the data," and to make the score not a limited amount dependent on finding one particular scheme but an unlimited amount related to the number and variety of patterns thus projected. I shall not seriously advocate this procedure, however, for two reasons. One is that a partial pattern for such extended plural answers is suggested in a preceding paragraph. Another is that the potential gain seems trivial in view of a second defect, absence of extended sampling.

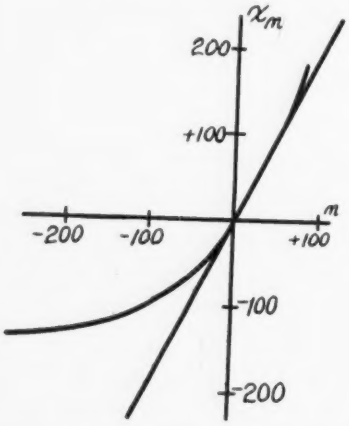

Fig. 3. Schemes A and E. Scheme E is based on fictitious data; curve resembles temperature-pressure curve for saturated steam.

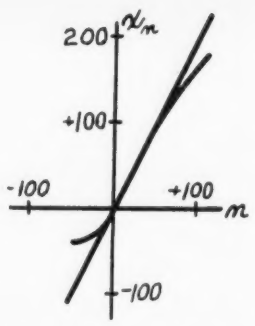

Fig. 4. Schemes A and F. Scheme F is based on fictitious data; curve resembles log exposure-density curve for photographic film.

Absence of extended sampling. Let us imagine possible reactions of test makers to my remarks about plural solutions and projections of schemes. They might shrug them off, thus, "In the first place, those schemes of yours seem scarcely justified by the data." They might continue, "Second, people can pretty well agree on a best answer. Don't you think a fellow would—well, I'll say it—would have to be crazy to propose some of those schemes?" "And third, by use of multiple-choice items with I.B.M. answer sheets, the subject is forced to limit his responses to those suggested."

The first comment might be interpreted as follows, "The given data are to be considered a fair sample and they contain no hint that the several terms in the continued series will do anything but continually increase in a linear way." Is this defense sound? Ideally, perhaps, a sample would be fair if it truly represented all the features of the population from which it was drawn. With an actual sample, there seems to be no way of telling whether or not the sample is fair in this sense, except by comparing the sample with the entire population. If the population is known well enough to make such comparison possible there would seem to be no reason for taking the sample. The reason for taking a sample is to provide information for making inferences about the otherwise unknown population. With actual samples, we rely somewhat on our method of taking them to produce samples that are fair. We do not rely entirely on such procedures, however, or on the accuracy of our reasoning, but rather we rely eventually on the verification of predictions—on whether or not extended samples agree with our inferences about the population. This has implications that will be considered later. At the moment we can say that the fair sample restriction was not contained in the instructions; further, that it would seem such restriction cannot be imposed.

Let us consider the second comment of our imaginary test makers, to the effect that right-thinking people can agree on a best answer. Is it well founded? Is it not the criterion of being socially acceptable, of conforming, of being orthodox? The criterion for acceptability of schemes in scientific investigation seems rather different and more complicated than the test maker suggests. When Arrhenius presented his brilliant doctoral thesis on dissociation of ionic compounds in water solution, his university condemned it with the lowest award possible without definitely refusing it—because his ideas were radically different from those of the scientific herd and of his professors. His thesis was neglected or actively opposed by several famous scientists during a period of years. Yet eventually the same thesis won him a Nobel prize in chemistry, medals from scientific societies, and assorted honors from his native Sweden (*28–29*). Darwin's theory of organic evolution has aroused opposition from some groups in society. The equation $E = mc^2$ was revolutionary for physicists when first proposed and even now seems revolutionary to the common man. As a first approximation, we may say that conceptual schemes should be consistent with recorded data and scientific ideas popularly held. But when more than one adequate scheme is available, choice among them is perhaps better made by observations and experiments designed to test the alternatives and by systematic simplicity rather than by orthodoxy.

The third view of our imaginary test maker was that proper selection of responses could force one answer to be better than any other. If we should admit it, we would seem to have prepared a cruel guessing game of trivial value. The rules of the game would be, "See if you can project into the given numbers the same pattern I chose to project

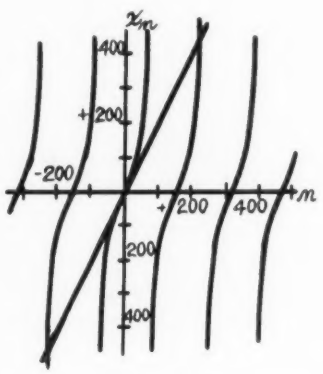

Fig. 5 Schemes A and G. Scheme G is $x_n = 100 \tan \frac{2n-2}{100}$, when n is in radians.

out of the infinity of patterns that fit the given numbers." When answers are to be extrapolated from the given numbers, any answer at all can be justified in terms of some scheme. Even interpolated answers suffer from the same difficulty, unless perhaps a requirement can be imposed concerning the degree of the equation or the shape of the plotted curve. Extending the given number series to seven terms—or a dozen—will not eliminate the difficulty. It is a logical fallacy to assume that a relationship that fits any given number of terms in a series must necessarily hold for all terms in that series; I may mention the difficulty of speculating successfully in stocks or in finding a winning system at roulette.

Test subjects cannot ordinarily request clarification of test items, cannot hope for consideration of plural solutions or of conditions attached to answers, and cannot profitably complain of error or injustice. Suppose a man is sophisticated enough to know that only one arbitrarily selected response will be credited when many are justifiable. Suppose a man realizes that the most acceptable basis for testing schemes is denied him, namely, extended sampling. These knowledges seem desirable, yet their possessor will be troubled. Shall he adopt the easy way? If he can guess what the test maker wants, shall he give it to him in an effort to score well and get into college or win the job? Or shall he be a conscientious objector? Shall he refuse to answer, on grounds such as those stated here, and suffer the consequences—no college or no job? Thus test makers, perhaps unwittingly, have proposed problems that were harder than they knew and were moral rather than scientific.

Routine procedures usually yield a solution. It will not comfort the test subject much to know that many solutions are possible if he cannot think of even one. How might he arrive at some answer? Perhaps he will employ a native ability to find patterns in data. But the items can be answered by procedures that are routine for sophisticated persons.

One routine procedure for attacking number series is that of successive differences, vanishing triangles, and figurate numbers (*30*). It may be applied to our series as follows:

Second differences:		0	0	0	
First differences:	2	2	2	2	
Original series:	0	2	4	6	8

The 409 test items in the 26 tests cited (*1–19*) include numerous duplications from test to test. Of the total, 190 items (47 percent) yield the keyed responses immediately by this procedure. In an-

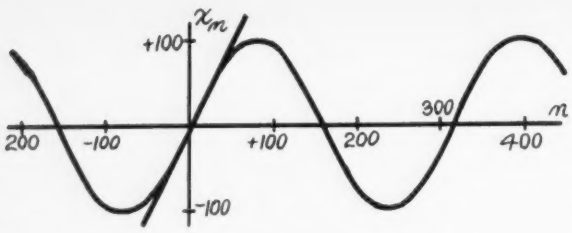

Fig. 6. Schemes A and H. Scheme H is $x_n = 100 \sin \frac{2n-2}{100}$, when n is in radians.

other 149 items (36 percent), the differences do not vanish, but use of the differences greatly simplifies finding an answer. Thus 83 percent of the items are solved directly or indirectly by use of successive differences.

I have regularly used successive differences on number series problems since my first experience with intelligence tests when I was about 12. Some of my fellow-students in high school physics laboratory used an adaptation of the technique, for example, to make the stretch of a spring come out in equal increments for equal additions to the stretching force or to find errors in their measurements. Neugebauer has used the technique in analyzing astronomical texts found on Babylonian cuneiform tablets (*31*). I have also used number series as a teaching device with several groups of college freshmen; in a total of perhaps 60 students, I have found at least three who used successive differences consciously and were able to explain the technique. They claimed to have learned the procedure in high school mathematics; it should be remembered that the Hogben work cited (*30*) is popular rather than erudite. It seems clear that at least a few subjects, some well below the genius level, know and use the procedure and may improve their scores considerably thereby. It might be interesting to investigate the procedures actually used by test subjects in answering number series.

Successive differences are the only routine procedure I know of that is likely to be useful in a testing situation. It is not likely that a subject would have time to sketch a curve showing one variable as a function of another (Figs. 1–9) and to read succeeding terms from the curve extended. Nor would it be feasible to use any of several techniques for finding empirical equations, but the techniques will be mentioned to illustrate the possibly mechanical nature of finding relationships among data already selected. One technique is using the shape of a curve as a guide to the form of an equation to represent it. Once the form of a first- or second-degree equation has been determined, the method of least squares can be used independently of the sketched curve to find proper coefficients for the several terms in the equation (*32*). For obtaining equations that approximate curves of higher degrees, methods of inspection and intelligent trial have been proposed (*33*). La-Grange's interpolation formula can be used to obtain a polynomial curve $y = Q^{(p)}(x)$ which coincides with the given curve at (p) points, as many as desired (*34*). A more straightforward procedure is provided by harmonic analysis and Fourier series (*35*).

> The Fourier analysis is suitable and complete for any curve whatever within the distance called one wave length . . . provided that the ordinate is finite and that the projection on the axis of a point describing the curve moves always in the same direction.

Coefficients for the several terms can be computed arithmetically or graphically, or they can be determined automatically by mechanical means (*35*) or by photoelectric and electric devices (*36*).

My remarks concerning routine procedures are consistent with the idea that any native ability to solve the items can be made more productive by a little training. Already we have coaching for New York State Regents' examinations and for College Boards. This may be a tip for mathematics and science teachers or for sponsors of student clubs in mathematics and science who wish their students to do well in "intelligence" and "scientific aptitude" tests.

Is it not curious that we have attempted to measure the quality of creativeness, or ingenuity, with items for which answers are known at all?

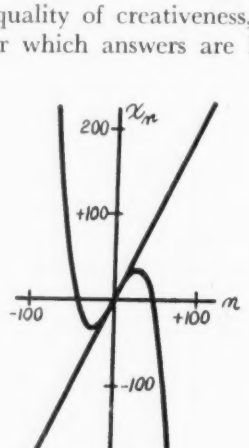

Fig. 7. Schemes A and I. Scheme I is $x_n = 2n - 2 - \frac{n^3}{1000}$.

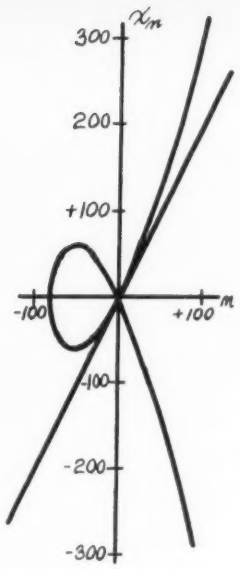

Fig. 8. Schemes A and J. Scheme J is $x_n = \sqrt{4n^2 + \frac{n^3}{20}} - 2$.

Is it not even more curious to have used many test items that yield to routine procedures? But how else could we have an answer key?

Validity for what? Routine procedures will usually give some solution for the test items that use number series; cultural bias seems to exist if some subjects, but not all, have learned such procedures. Further, such test items seem not to be valid, because (i) it seems impossible to prevent plural solutions that arise because patterns are projected into the data rather than are inherent; and (ii) test subjects are denied what seems to be the best way of choosing among the many patterns, namely, making predictions based on a particular pattern and checking the predictions against extended samples.

Perhaps we should ask what kinds of evidence can be offered to prove that items that employ number series are valid for testing ability to find patterns in data. It has been suggested that people who have a lot of ability to find patterns in data can be found by the following external criterions: (i) estimates by instructors of students' class ranks in aptitude for scientific research (*24*), ranks in "research potential" (*37*), and ranks in "reasoning ability" (*4*); (ii) success in Westinghouse Science Talent Search, getting a job in applied science or in scientific research, going on to work for a doctorate in science, or winning a great distinction such as a Nobel prize (*37*); (iii) students' grades in mathematics, science, engineering, and other courses (*4, 19, 23, 24*); and (iv) "those job activities that are characteristic of effective scientific personnel" (*38*). However, attempts at correlation with external criterions such as these run into a major difficulty. Although the finding of patterns in data can be more or less routine in the limited problems offered in these test items, it is still essentially a creative act and particularly so in scientific research. It seems as unpredictable, as little responsive to demand, as is the creation of a masterpiece in painting or symphonic music. A person may have demonstrated repeatedly considerable ability to find patterns in data and still, in any particular problem situation, may fail while another with lesser reputation succeeds.

A second attempt to establish validity of these items consists in showing that intelligent people do well on them. Here the attempt is to show that scores on the items are consistent with scores on the battery of which they are part and that total test scores correlate highly with scores on other tests of mental ability that have already been generally accepted (*2, 3, 5, 7, 10, 13, 15, 16*). Factor analysis has been used in efforts to find out what factors the several tests of a battery have in common (*18*), but pure tests for only one factor have not yet been devised, and it seems difficult to interpret conceptually any of the factors derived statistically. For example, to my knowledge, no statistical factor has been shown to correspond with ability to find patterns in data.

A third kind of validity is important to the success of a testing program. Test subjects should feel that the items—and total test situation—are going to do the job they are supposed to do. A superficial resemblance between the test situation and a phase of scientific research might lead some subjects to accept the situation in principle. But the man with the greatest ability to propose solutions might be the one most frustrated by the absurdity of not being able to test any of his hypotheses by taking extended samples.

A fourth kind of evidence for validity is found

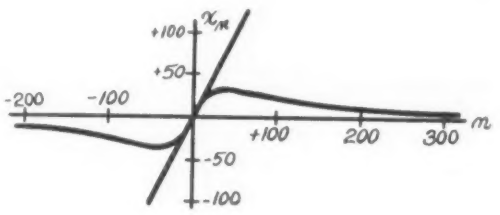

Fig. 9. Schemes A and K. Scheme K is $x_n = \frac{2500n}{n^2 + 1250} - 2$.

in the considered statements of expert critics that items are sound, just, and well-founded—or that they are not. It would seem that the points made here should be considered in any critical evaluation of the test items that involve finding a pattern in data. Although test items that employ number series have been widely used—are part of the stock-in-trade of the test maker—it is here suggested that the items are not valid and that further use of them is questionable.

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SCIENCE ON THE MARCH

LOBUND COMES OF AGE

AFTER an inconspicuous beginning in a corner of a dean's laboratory and a long and difficult period of growth, nourished by the pioneering skill and courage of its founder and the vision of those who supported it, the Laboratories of Bacteriology, University of Notre Dame (LOBUND) has come of age. In attaining its majority LOBUND has become world-renowned for research in the life sciences.

After its early struggle for existence, LOBUND during its youth received a great stimulus in the form of outside support. Chief among these agencies that foresaw the role LOBUND would play in life sciences research was the infant Office of Naval Research. After ONR's establishment in 1946, its scientists quickly realized the contribution LOBUND could make in solving many problems that plagued the Navy. Accordingly, ONR provided financial support that enabled LOBUND to grow.

The father of LOBUND was a young undergraduate student at Notre Dame, James A. Reyniers, who in 1928 became interested in bacteriology. Since the unit of the science of bacteriology is the cell, Reyniers first attempted to discover the nature of the cell. To do this he had to develop instruments to isolate the cell.

Another pressing problem was a workshop where the necessary instruments could be developed. The Reverend Francis J. Wenniger, gave the young bacteriologist a corner of his own laboratory in which to work. Father Wenniger became so interested in the work that in 1930 he persuaded Reyniers to study for a graduate degree and offered him an instructorship. In addition, Father Wenniger arranged for Reyniers to conduct his experiments in a small room in Science Hall.

Working with very limited funds, Reyniers struggled along until he succeeded in piecing together equipment that would permit him to study a single bacterial cell under a microscope. Success, however, was not within his grasp, for unwanted bacteria kept entering the preparations. His next problem, therefore, was to find a way to keep them out.

More apparatus had to be constructed. To check its tightness Reyniers tried to raise germ-free animals—animals free of all bacteria and disease-producing viruses. Generally, before birth an animal is germ-free, but various microorganisms enter the system during and immediately after birth. Only in an artificial environment, such as that developed at LOBUND, can animals be raised completely free from microorganisms of all kinds. Several Notre Dame instructors became interested in the project and worked with Reyniers in developing techniques, and they persevered until by 1934 germ-free animals were being raised successfully.

LOBUND continued to grow slowly. In 1937 it moved into expanded quarters—the entire first floor of the new Biology Building at Notre Dame. Gradually scientific organizations saw the value of research with germ-free animals and began to contribute to LOBUND's support. In 1939, in cooperation with the Zoller Memorial Dental Clinic of the University of Chicago, LOBUND began one of its well-known studies—the causes of tooth decay.

When the Office of Naval Research undertook to provide financial support for continued research at LOBUND, the staff was expanded and the research program was enlarged. The university helped by financing construction of three new buildings: the Germ-Free Life Building, a machine shop, and an animal colony house. At the dedication in June 1950 the Rev. John J. Cavanaugh, president of Notre Dame, announced the establishment of LOBUND as an Institute for Research in the Life Sciences. Thus LOBUND came of age.

Already quarters are bulging at the seams and in need of expansion—LOBUND's growth must continue. The small staff, approximately 50 men and women, work tirelessly on the three parts of the program: germ-free life, biological engineering, and micrurgy.

To date the raising of germ-free animals is the most widely known of LOBUND's activities. Two methods are employed to insure that animals are germ-free at birth: (i) fertile eggs are washed in a germicidal bath, then placed in germ-free cages where they are allowed to hatch; (ii) investigators working through rubber gloves attached to the walls of the sterile cages deliver the young of mammals by Caesarean section.

After birth the animals continue to live in sterile cages. Technicians using arm-length rubber gloves sealed into the sides of the cages feed and care for the animals. A glass window in the top of the cage

permits the technicians to observe the animals. Rats, chickens, mice, guinea pigs, monkeys, rabbits, and even insects can now be raised germ-free.

A study of these animals furnishes many useful data. Eventually scientists will be able to test the theory that the microorganisms inhabiting a human being or animal are harmless. First one, then another microorganism, and finally, combinations of microorganisms can be introduced into the germ-free animal and the effects noted. Significant advances in microbiology are anticipated.

Tooth decay affects 98 percent of the people of the United States, and in its advanced stages may cause serious illness. Because so many man-hours are lost to the Navy from dental caries each year, the Office of Naval Research is interested in its exact cause. Germ-free animals that are fed a caries-producing diet develop no tooth decay, whereas normal animals fed the same diet develop caries. By adding first one microorganism, then another, scientists hope to find the caries-producing microorganism.

Other problems under investigation are the role of intestinal microbes and nutrition; radiation sickness; protein metabolism; heart disease; virus infections; and forms of cancer, including a disease in chickens that resembles cancer in man.

Although the Germ-Free Life Division is best known, the other divisions are also important in the study of the life sciences. The Biological Engineering Division, in contrast to the Germ-Free Life Division, attempts to keep germs in, instead of outside, the cages. Here technicians need have no fear of developing any diseases under study, for complete protection is provided. Since the microorganisms cannot escape the units, one great hazard in their study has been overcome. Still another advantage of this method is that unwanted germs are kept out of the units. Large amounts of a single type of germ, therefore, can be grown.

A scientist can safely study the problem, for example, of air-borne tuberculosis infection in guinea pigs. Animals are placed in the units, and the bacteria are sprayed into the air reaching the unit. Technicians care for the animals by means of built-in rubber gloves and observe them in complete safety through a sealed glass window. After the experiment is concluded, the animal is removed through a sterile lock. Steam under pressure kills the disease-producing germs in the cage, which then is ready for the next experiment.

Not only can scientists study various bacteria that cause specific diseases, but they can also study vaccines to combat the diseases. The production of a vaccine requires large quantities of bacteria of the desired strain that are free from contamination by other strains, and LOBUND's sterile units are particularly adapted to the process of growing them.

The third addition to LOBUND was the Micrurgical Division. Micrurgy means microsurgery, or surgery under a microscope. With the development of special tools it became possible to operate on a single cell—an important advance for the study of living cells.

At present only germ-free insects, notably flies and cockroaches, are under study, but eventually germ-free mammals may also be investigated.

Two studies are currently being made. To date more work has been done on the first—the relationship between microorganisms and the individual cells of plants and animals. Bacteriologists have found that insects are often immune to bacteria that produce disease in man. In some cases, in fact, the insects actually thrive on the bacteria. The Micrurgical Division hopes to find out what causes this condition and then work out a method for controlling diseases in man. The second study, which concerns the part that cells play in heredity, should produce results of value to the study of the life sciences.

At the spring meeting of the Society of Illinois Bacteriologists the father of LOBUND received the annual Pasteur award. Established in 1948, this award honors annually an outstanding midwest bacteriologist for his contributions to microbiology. According to J. C. McCaffrey, award committee chairman, Reyniers was selected "for his pioneering work in germ-free research."

BETTY APPLETON

The more we do, the more we can do; the more busy we are, the more leisure we have.—WILLIAM HAZLITT.

BOOK REVIEWS

Snow Crystals: Natural and Artificial. Ukichiro Nakaya. Harvard Univ. Press, Cambridge, 1954. xii + 510 pp. Illus. $10.

SURELY the study of snow crystals is one of the most fascinating topics available for a scientist, and snow has been around waiting to be observed for a long time. Yet until this century only the most rudimentary classification of crystal types had been made, and no systematic research into the mode of growth of the various crystal types had been attempted.

A generation ago the name of Bentley was automatically associated with snow crystals. Any decent photograph of a crystal at that time was most likely one of the 6000 photomicrographs taken by him during his lifetime. Unfortunately, Bentley's beautiful photographs contributed little of use to a scientist, since he concentrated on only the most photogenic crystal types, ignoring others that occur just as frequently. Furthermore, he apparently failed to record magnifications or temperatures.

In this generation the outstanding name in the field of snow-crystal studies is Ukichiro Nakaya. Since 1932, Nakaya and his coworkers have compiled and analyzed a huge collection of data and have solved the major problems of the development of the various crystal types. In this book, Nakaya gives a detailed account of the observation and analysis conducted under his direction, culminating in the successful artificial production of snow crystals of any type. The book includes more than 1500 photomicrographs of crystals of all types, both natural and artificial, and in the case of artificial crystals, in all stages of growth. The observations and experiments, of course, all had to be done at temperatures below freezing, and the technique of handling tiny crystals and photographing them through a microscope is one demanding the greatest patience and perseverance.

The first part of Nakaya's book deals with the observation of natural crystals and illustrates the myriads of forms in which snow crystals occur. Because plane hexagonal crystals are usually shown in a representative photograph of a snow crystal, one is misled into thinking that these are the most common type. Actually, spatial and irregular crystals fall in greater quantity. Interesting, too, is the fact that the largest crystal observed is about 9 mm across, while most probable dimensions are anywhere from 0.25 to 4 mm, depending on the type. Measurements of the rate of fall of individual crystals show that plane and spatial dendritic and powder snow particles fall at a fixed speed, regardless of their size, a finding not to be expected.

The second part of the book describes the various attempts at production of snow crystals in the laboratory. The biggest difficulty encountered in attempting to grow isolated snow crystals was to get them to grow somehow suspended in the air. When filaments of cotton were tried, crystals grew all over them and were so closely packed together that growth of an individual crystal could not be followed. A well-dried rabbit hair was finally found to have just few enough irregularities, or projections, on it to grow isolated crystals. This hair was firmly mounted at the top of a cylindrical chamber where the air temperature was T_a. This air had been supersaturated by passing it over water at a higher temperature T_w. By controlling T_a and T_w, every sort of crystal could be grown; that is, crystal type was found to be a function of both temperature and supersaturation. These temperatures, T_a and T_w, plotted against time formed a history of development of the snow crystal. A particular point on a graph of T_w against T_a (or supersaturation with respect to ice against T_a) defined a particular crystal type. One of the most satisfying rewards of the workers in Nakaya's group must have been the artificial production of crystal types that had never been found in natural snow and the subsequent search in which these types were eventually observed.

This book is certainly unique. It is unlikely that as complete a record of snow-crystal types will ever again be compiled. The hundreds of magnificent photographs and the exceptionally handsome format make it an outstanding addition to any scientist's library.

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The Western End of Lake Erie and Its Ecology. Thomas H. Langlois. J. W. Edwards, Ann Arbor, Mich., 1954. xx + 479 pp. Illus. $10.

WHETHER optimism prevails in discussions that appraise the achievements of fish management or whether the discussants dolefully stress human blunders, shortsightedness, and unenlightened self-interest, one thing is generally agreed upon in such sessions. Thorough synecologic studies should precede conservation measures aimed at the improvement for human use of the quality or quantity of aquatic resources.

Langlois' book is the most extensive record to date, dealing with the total ecology of an intensively used large body of fresh water; this alone would make it an important new book. Studies of the past 60 years are summarized. The author spent almost 30 years in personal contact with problems of fish management in Ohio and more specifically with questions of the productivity of Lake Erie.

Fluctuations in year classes of certain species of fish, at times leading to great abundance, at other times leading to extreme scarcity, are usually ascribed either to overexploitation or to causes that eventually rest on climatic changes. More gradual shifts in species composition, however, are most certainly due to preceding changes in the physical or chemical properties of the water. Langlois' arguments for turbidity as the key

factor in the productivity of western Lake Erie—itself the key area of the entire lake—are well presented and convincing.

Abundance of food for bottom invertebrates and fish is the result of abundant plankton crops; these crops in turn are shown to depend on the yearly fluctuations of the silt load that the rivers of Ohio's rich agricultural sections deliver to the lake in its western end. The spawning and hatching success of a number of fish species is also closely connected with the silt and clay loads that these tributaries carry at certain crucial times. These streams are now without the buffers of their former large estuarine swamps in which the suspended matter could settle out before reaching the lake itself.

The book is very explicit in tracing back to the land what happens in the water. These events are the results of man-made changes in the course of more intensive land use and denser occupancy. The long-term management proposals that are presented deal, therefore, with the soil on the water sheds rather than with manipulations of the fish themselves.

Much that is valuable beyond local interest is contained in the inventory of flora and fauna. It is unfortunate, however, that bits of fish natural history are scattered under three different headings. Discussions of breeding and feeding habits could have been unified with the collection records. This would have been welcome to the layman without disturbing the biologist.

It is a pity, although hardly the author's fault, that the illustrations are poorly reproduced. If sketches had been included in the descriptions of trap- and pound-netting they could have amplified, or even replaced, many well-chosen words. Unfortunately there is no good map.

Noteworthy sections are the clear and exhaustive treatments of freezing of a lake ("The ice sheet") and parts of the general summary where the productivity of standing waters is most ably discussed. The large bibliography is especially valuable in a book that deals with a relatively large area and covers an extended period of time.

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A Brief Text in Astronomy. William T. Skilling and Robert S. Richardson. Henry Holt, New York, 1954. viii + 327 pp. Illus. $4.

ON the thesis "that a majority of college students have time for no more than a one-semester course in the subject," the authors have written a textbook that is brief, readable, and comprehensive, with excellent integration of the complex subject matter of astronomy. The marginal memoranda of the contents of each paragraph add to the beauty of the composition and offer a ready means of locating desired information. The comprehensive explanatory sketches and the really wonderful celestial photographs challenge the reader's interest. Concise tables for reference with a key to the pronunciation of the Latin names are featured. Well-chosen questions and exercises at the end of each chapter are an aid to careful reading. A list of books recommended for supplementary reading and a well-printed and carefully arranged glossary should be most stimulating. The star maps with a brief discussion of the features of each month's seeing are large and clearly printed so that they should provide easy reading in the rather dim light usually available when they are used. Here, again, the pronunciation of star names is clearly indicated.

I know of no other textbook that so cleverly combines so much exact scientific information with such interesting discussion on so vast a subject. The authors, already well known for their outstanding textbooks on astronomy, present in this new book an expertly chosen collection of the most important facts about astronomy. With this book available, there is no longer any excuse for omitting a cultural course in astronomy from the standard liberal arts curriculum.

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The Neolithic Cultures of the British Isles. Stuart Piggott. Cambridge Univ. Press, New York, 1954. xix + 420 pp. Illus. + plates. $13.50.

IN this comprehensive survey, Stuart Piggott presents for the first time "a detailed account of all the aspects of the material culture of a complicated phase of British prehistory, rather than a treatment of a selected element such as pottery or tomb types." His hope that, for Britain, this book will serve as a sequel to Clark's *Mesolithic Settlement of Northern Europe* seems admirably fulfilled.

His fourfold problem comprises the definition of content, relationships, and geographic distribution of numerous culture groups; the establishment of regional time sequences; the establishment of relative chronologies between regions; and finally, the linking of the British Neolithic cultural traditions and chronologies with those on the Continent. Roughly the period covered falls between 2000 and 1500 B.C.

He divides the British Isles into six major and several minor regions of diversity, considering as primary Neolithic cultures those that are intrusive and unmodified and as secondary Neolithic cultures those that show strong modification from surviving Mesolithic traditions. This picture is further complicated by necessary time divisions into Early, Middle, and Late Neolithic phases, although earlier culture groups apparently survived and coexisted with later ones.

The earliest Neolithic cultures stem from the West European tradition. The widespread Windmill Hill culture is known from causewayed camps, habitation sites, flint mines, axe factories, and unchambered long barrow graves. The other groups of this phase are not so well defined.

The Middle and Late Neolithic phases are character-

ized by the influx and spread of Megalithic people who practiced burial in collective chambered tombs. With Childe and Daniel, Piggott recognizes two main classes, passage graves and gallery graves. He further subdivides these into several types and derivatives through which he traces the course of settlement.

The secondary Neolithic cultures fall for the most part in the Late Neolithic phase. Mesolithic survivals appear primarily in the stone work. Of these, the Peterborough and Sandhill groups show circumpolar traits in their stone industry and relationships to the Baltic Pit-comb and cord-marked Funnel Beaker groups in ceramics. The distinct Rinyo-Clacton group has its nearest ceramic affinities with western France and perhaps the Iberian peninsula. The Dorchester group, which includes the monuments of the Henge type, is more shadowy as a complex and may overlap the Wessex Bronze Age.

In commenting on the provisional character of his chronological table, which organizes the content of this very complex, detailed, and workmanlike study into a readily understandable form, Piggott feels that, although it is open to criticism in detail, the broad pattern is sound. There are, I think, few who would disagree. For the student of prehistory this volume competently fills a gap of long standing and is most welcome.

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V-2. Walter Dornberger. Trans. by James Cleugh and Geoffrey Halliday. Viking Press, New York, 1954. xviii + 281 pp. Plates. $5.

WORLD War II produced three major engineering achievements, the atomic bomb, the development of microwaves (radar), and the V-2 high-altitude rocket.

A large number of books and articles have described in detail the scientific, engineering, and speculative aspects of the V-2, but none have presented more than hints about the political, bureaucratic, personnel, and organizational problems involved in the project.

Dornberger, a general in the German Army, was head of the high-altitude rocket project from 1930 to 1945. Most of the development work was carried out at Peenemünde rocket station on the Baltic. This book, written from personal notes and diary, has an authentic ring. There are short accounts of several types of military rockets, but the bulk of the book describes the development of the A-4 (Aggregate 4), later called the V-2 (Vengeance weapon 2), into a highly successful military weapon which cannot be intercepted even today.

The Peenemünde rocket station staff had to fight against skepticism for years. They were handicapped by insufficient equipment, materials, and personnel. However, as soon as the feasibility of the V-2 had been demonstrated, they then had to fight industrial leaders, politicians, and the Nazis to keep them from taking the whole project away from the staff. One is impressed by the fanatical devotion of the Peenemünde staff to their work. Dornberger always kept the military use as his primary goal, but his staff, particularly Werner von Braun, kept the goal of space travel in the forefront. The staff appeared to be politically naive and uninformed about the full implications of the National Socialists and the war raging about them in the latter stages of their work.

This is a very readable book. It is important because it demonstrates the methodology of research and development necessary under a totalitarian regime. It is a must for anyone interested in rocketry.

THOMAS S. GARDNER
Rutherford, New Jersey

Ways of the Ant. John Crompton. Houghton Mifflin, Boston, 1954. 242 pp. Illus. $3.50.

HERE is a book about ants written by a layman for laymen but also worthy of the attention of specialists. I enjoyed it; so did my 12-year-old son. And if such a book can delight both a callow youth and a hard-bitten myrmecologist, it must be good.

The topics treated are well chosen: life-cycle, colony founding, nest, the house ant, slavery, thief ants, guest ants, relations with aphids, harvesters, fungus growers, warfare, driver ants and army ants, guests, and intelligence. There is also a long chapter on termites. The eight full-page pen-and-ink drawings by J. Yunge-Bateman are accurate, artistic, and useful.

But what sets this book apart is the author's style. I can easily describe it as readable, interesting, absorbing, and even fascinating, but these adjectives do not tell all. There is something more that is difficult to characterize. Perhaps I can explain it in this manner: Crompton was interested in ants. He desired to communicate this interest to laymen, and obviously he enjoyed writing the book. Nevertheless, he did not take either himself or his book *too* seriously, hence the light touch of humor—not ponderous, not frivolous, just delicate. To illustrate: ants, unlike bees, do not regard ant eggs as sacred; ". . . they believe that eggs were made for ants and not ants for eggs, and will eat them when hungry without hesitation."

The author uses the vernacular name of an ant whenever there is one, but he does not avoid scientific names by coining new names in English. This would scarcely deserve commendation were it not for the fact that in another recent popular book about ants the author has coined numerous abominations by translating the scientific names into English.

With so much of merit, I regret having to mention a few faults. Typographical, grammatical, biological, and entomological errors occur, but they are relatively few and not too important. Somewhat more numerous and more serious are the myrmecological errors. Most serious of all perhaps are the semantic sins. Ants *decide, enjoy, prefer, think, know, wonder why, understand,*

and so forth. To be sure, Crompton cautions the reader in the preface against interpreting ant behavior in human terms. But I cannot agree that the use of such terms is legitimate unless they are put in quotation marks or qualified by "seem to" or "appear to." Furthermore, such loaded words as *shocking, degenerate, vice, abnormal, unnatural, debauchery,* and *orgy* are used in discussing the normal behavior of parasitic ants.

If this work runs into a second printing (and I hope it will), perhaps the publishers will make the corrections that such excellence deserves. In the meantime, I am happy to overlook the faults and recommend it to laymen, to biologists in general, to entomologists in particular, and even to myrmecologists. May they enjoy it as much as I did!

GEORGE C. WHEELER
Department of Biology, University of North Dakota

Math Is Fun. Joseph Degrazia. Emerson Books, New York, 1954. 159 pp. Illus. $2.75.

THIS is a collection of about 200 "mathematical brain-teasers." All the problems are stated so as to be understandable by anyone with a knowledge of arithmetic. Most of them can be solved by simple, but sometimes tricky, arithmetic reasoning, some need a bit of high school algebra, a few border on the "theory of numbers," none are geometric. Each of the 17 chapters concentrates on a particular type of problem. The first one or two problems in each chapter are familiar, and they are followed by other problems devised or collected by the author. Solutions are given at the end of the book.

S. B. MYERS
Department of Mathematics, University of Michigan

Main Currents of Scientific Thought. A history of the sciences. S. F. Mason. Abelard-Schuman, New York, 1954. viii + 520 pp. $5.

THIS book seeks to trace the development of certain aspects of various sciences from their beginnings in the ancient civilizations of Babylonia, Egypt, Greece, and Rome, through Oriental and medieval cultures, to modern times, with special emphasis on "The scientific revolution of the sixteenth and seventeenth centuries," the 18th century "Development of national scientific traditions," 19th century science as "The agent of industrial and intellectual change," and, finally, the 20th century's, "New fields and new powers."

The author views the growth of science as stemming primarily from two traditional sources: practical experiences and skills and speculative aspirations and ideas. His personal views are outlined in the final chapter with its provocative title, "Science and history." Here he defines his own usage of the word *science.* Here, too, he briefly summarizes arguments and evidence for his thesis that "in the modern world science has led mainly to the secularization of thought and development of utilitarian applications." Here he concludes with a confession of his own hopes and fears in view of the facts "that science has become one of the important determinants of the strength of any major historical movement during the twentieth century."

The general reader will be pleased with the attention given to novel subjects not usually found in a history of science. For example, one finds chapters headed "The scientific revolution and the Protestant Reformation," "German nature-philosophy," "Science and engineering," "Science and the national movements in Italy and Germany," and "Some aspects of American and Soviet science." Nonscientists will also welcome information on such topics as "The science of India," "Descartes: The mathematical method and the mechanical philosophy," and "Astrophysics and the theories of world structure." Scholars, however, may regard quite critically the choice of particular individuals (both scientists and nonscientists), as well as the selection of certain fields of science, for popular interpretation. Even the general reader will be conscious of overtones of the author's personal philosophy of science.

Historians, indeed, may well question the validity of the so-called "main currents." Sociologists may not be satisfied with the evidence adduced for the guiding emphasis on the motivation of science primarily by utilitarian needs and by national objectives. Professional philosophers will note the failure to present modern scientific thought in its various popular roles in positivism and in neo-Thomism (*et al.*). Theologians may be concerned with a failure to discriminate between the formal relationships of organized religious bodies and the expressive growth of spiritually minded individuals —not to mention a possibly undue emphasis on Protestant traditions.

Even scientists may have some misgivings about potential inferences. Thus, life-scientists may view with alarm the disproportionate material on the physical sciences in the 20th century. Psychologists may well wonder at a history of the sciences that pays so little attention either to psychology *per se* or to its interrelationships with all other sciences. Mathematicians, too, may note with dismay the failure to pay any homage at all to mathematics as a handmaiden of the sciences—if not as the queen. Being a theorist, I may have been unduly sensitive to the author's apparent lack of appreciation of the nature and role of theory in science. It is to this bias that I ascribe his somewhat unusual interpretations of the significance of men like Archimedes and Galileo as well as Francis Bacon.

I am personally disturbed by the author's one-sided representation of science as a national instrument in view of the more prevalent regard for science as an international leavening. Perhaps his attitude is merely a reflection of the technologic struggles of our own times. I do believe, however, that a neutral account of matters is requisite until further scholarship vindicates the author's own value judgment. I find it particularly regrettable that the author finds it necessary to make a

pointed comparison between present-day American science and Soviet science. So much political propaganda conceals the latter; so much commercial advertising distorts the former. A comparison of the philosophies and sociologies of science of the two cultures might be more in order here—but even these are hazy amid the fog of ignorance and the smog of distrust. More basically, however, in what sense is it logically justifiable to distinguish between American science and the science of any other country? To be meaningful at all, one seems to be committed essentially to a general utilitarian concept of science or to a particular philosophic interpretation of science. Certainly most persons engaged in uncommitted, pure science would disagree with the author's statement that "a practical application, the atomic bomb, is commonly regarded as the most important single product of American science during the present century, while the theoretical discussions concerning the fundamental theories are regarded as the most significant of the developments in Soviet science." Regarded by whom? By Americans? No! By scientists? No! So significant a conclusion should be carefully substantiated.

It is inevitable that any book aiming to cover so vast a field of knowldge cannot escape errors—sometimes of fact, sometimes of interpretation. No brief book review should attempt a detailed listing. Much more important, however, are the suggestive inferences that may be drawn by an unsuspecting reader. One of these occurs with respect to the work of the National Science Foundation in the United States.

After a single sentence about the establishment of the Foundation in 1950, the author comments as follows:

> Even here the American practical tradition asserted itself, for a clause in the Act specified that the Foundation should initiate and support such applied research of a military nature as might be requested by the Secretary of Defense.

This being the sole discussion about the National Science Foundation, a false impression might ensue about its operation and function. To be sure, this clause does occur—among many clauses dealing with the responsibilities of the Foundation—but it is hardly a representative one. To date, indeed, there has been no occasion for its invocation. Meanwhile, the Foundation has carried on a steadily increasing program that is more properly described by the first clause of the section cited, namely, "to develop and encourage the pursuit of a national policy for the promotion of basic research and education in the sciences." Basic research is the primary responsibility of the National Science Foundation—not applied science!

In fine, the uninformed reader has to be continuously on guard against presuppositions in analyses throughout this book. On the whole, however, it is worth reading. It will undoubtedly stimulate further research on the hypothetical answers given to important questions that are raised.

RAYMOND J. SEEGER
National Science Foundation

World Geography: An Introduction. Loyal Durand, Jr. Holt, New York, 1954. vii + 372 pp. Illus. $5.25.

THIS college textbook is concerned mainly with description and analysis of the physical elements of geography. The major landform, soil, and climate types of the world are succinctly described and well illustrated. The sections devoted to the human relationships of these elements, particularly those devoted to types of agriculture, are less satisfactory. Tropical agriculture is overemphasized, grazing of tropical grasslands is slighted (there is, for example, no mention of the influence of the tsetse fly or of "cattle cultures" in Africa), and little conception is conveyed of the relative areal or productive importance of the several Mediterranean crop associations. The statement regarding western European agriculture, that "dairying predominates in the coastal edges and in lowland England, and commercial livestock and crop-farming in the interior," also seems hardly adequate. Treatment of nonagricultural geography is limited to chapters on mining and on world population distribution.

Since the fine crop of veterans that blessed every college for some years after the war began to dwindle, publishers' representatives have been telling us that colleges and universities were calling for a simplified, shorter, easily readable, more highly illustrated textbook in geography. Here is such a book. But one hates to believe that college students require to be told that "small areas of land, surrounded by water, are islands" and that "human beings are domiciled on land surfaces."

WILLIAM A. HANCE
Department of Economic Geography, Columbia University

Theory and Method in the Social Sciences. Arnold M. Rose. Univ. of Minnesota Press, Minneapolis, 1954. xii + 351 pp. $5.

THIS volume includes the 1952 AAAS prize essay in sociology, together with some other previously published and unpublished materials by the same author. These essays are grouped under social theory, values in social research, contributions of sociological theory to the other social sciences, methodological issues in sociology, and some specific techniques of sociological research. Rose discusses such problems as social organization and disorganization; the role of the mass, the audience, and the public in modern society; and the contributions of sociology to the functioning of the individual and of voluntary organizations in the mass society, to race and culture conflict, and to the solution of group conflicts. The first group of essays summarizes the common thread of social theory that has been developed in the uncoordinated traditions of sociological thinking and research of French, German, and American writers on social structures and social problems.

The essays on values in social research deal with the selection of the problem for research as a major step

in any research undertaking and draw a distinction between objectivity and neutrality. They stress the need in social science for clear recognition of the presuppositions that go into the selection and definition of the problem for study. Although Rose would not orient social research primarily to social action, he emphasizes that problem rather than method is primary in social research and that the orientation of theory provides the meeting ground for these divergent approaches which have much to contribute to each other. The chapter on "Social responsibility of the social scientist" extends the usual concern with social control of the new physical world to the control of social knowledge. The use that has been made of the findings of social science in such fields as propaganda, industrial relations, and economic control poses problems not unlike those that have been more widely discussed in relation to physical and chemical developments.

The discussion of the methods used in social science and the argument for greater use of the deductive ideal-type method and for more rigid statements of the truisms that have relevance and predictive usefulness indicate areas for fruitful development of methods in addition to those that have been more widely used and more intensively developed.

These essays succeed in summarizing concisely and intelligibly a large body of tested knowledge that is the basis of sociological theory and in analyzing methods that can be used further to develop and test such knowledge. The result is useful both to the specialist in this field and to the nonspecialist who is interested in the development of scientific methods in this area of study.

CONRAD TAEUBER
Bureau of the Census, U.S. Department of Commerce

Sea-Birds. An introduction to the natural history of the sea-birds of the North Atlantic. James Fisher and R. M. Lockley. Houghton, Mifflin, Boston, 1954. xvi + 320 pp. Illus. + plates. $6.

FISHER and Lockley love sea-birds, and the sea cliffs, beaches, and especially the lonely rocky islets where sea-birds gather and breed, sometimes in millions. They have visited every "good British sea-bird cliff or island" and many others from the Salvages to Spitzbergen and Iceland. The mere tale of these localities reads like a saga. Nor have they been content to revel in the spectacles of birds swirling in myriads about giant crags above turbulent seas and to write a "list" of their names. They have probed deeply and particularly into the problems of sea-birds: their evolution, the effect of man (important predator and benefactor) on their numbers and lives, other influences controlling their numbers, their travels (which may be from one end of the world to the other) and navigation, and their social and sexual behavior.

The authors plan to continue this study in future years, revisiting localities and exploring others. Here they have simply "paused in field-work" to give the results of their studies and what they have gathered from "five thousand original notes, papers, and books." They give only sketchy descriptions of birds and eggs, but there is much about the lives and ways of North Atlantic sea-birds and indications of what is left to learn. The 77 photographs of birds (nine in color) and Peterson's paintings truly illustrate the book, and the 66 maps and diagrams aid in understanding it.

CHARLES H. ROGERS
Princeton Museum of Zoölogy, Princeton, New Jersey

The Meaning of Social Medicine. Iago Galdston. Harvard Univ. Press, Cambridge, 1954 (For the Commonwealth Fund). viii + 137 pp. $2.75.

SOCIAL medicine as a distinct discipline takes into account all factors affecting health and the impact of these factors on the over-all optimum functioning of society. Galdston clearly and comprehensively reviews the various viewpoints presented by many authors on the subject of social medicine. An attempt is made to arrive at a precise and definitive definition, an attempt that is, on the whole, successful in presenting the concept of social medicine, but is less successful in elucidating simply and clearly the boundary lines between it and related fields of medicine. What social medicine is not is stated masterfully and emphatically. It is not bacteriology, public health, preventive medicine, or socialized medicine. The differences between social medicine and socialized medicine are lucidly and specifically stated. That social medicine parallels and, in many instances, overlaps public health and preventive medicine is admitted, but it is also different from both.

One chapter is devoted exclusively to the "failures" of modern medicine, in which it is argued that modern medicine has "failed in the elimination of disease and in the promotion of health." In this golden age of therapeutics, emphasis is placed on a specific etiological diagnosis and a specific therapeutic regimen. The author believes that too little attention is directed toward determining the reasons why one person or group of persons becomes ill, while another person or group of persons remains unaffected by the illness. Moreover, little effort has been expended on the evaluation of the impact of an illness on the individual and his functions as a member of society.

Galdston particularly directs attention to such diseases as cancer and rheumatism, which have not been susceptible to current therapy. However, he overlooks the fact that some diseases, such as syphilis, which at one time had tremendous significance to the skilled practitioner of social medicine, are no longer important diseases in this field because of the discovery of highly effective therapeutic agents. On the other hand, he indirectly implies that the field of social medicine becomes more limited as new specific therapeutic agents are discovered by stating that psychiatry perhaps exemplifies the best current medical practice in the area of social medicine, an area of medical practice in which there are few specific therapeutic agents available. It is

undoubtedly true that, since medicine has become more "scientific," less attention is being given to the environmental aspects of disease as well as to the psychological factors involved in an acute or a chronic illness.

A strong plea is made for a change in our present medical curriculum to the end that emphasis be placed on the maintenance of an optimal state of health rather than on the more limited field of diagnosis and treatment of disease. Although many medical educators heartily agree with this philosophy of approach, the practical implementation of such a program has presented many almost unsurmountable obstacles. Medical education, as well as the practice of medicine, appears to many to be only a facet of a broader area of social medicine. Although social medicine is largely neglected in the current medical curriculum, the author has not clearly and precisely outlined the responsibilities of the medical educator and the practicing physician in this broad discipline.

After reading this scholarly exposition on the "meaning of social medicine," one is left with the feeling that a sequel should be written, outlining possible practical approaches that would indicate to the physician how he could better serve his patient and society to the end that the health of the individual, as well as of the community, would be improved.

ALLAN D. BASS

Department of Pharmacology, Vanderbilt Medical School

How Strong Is Russia? A geographic appraisal. George B. Cressey. Syracuse Univ. Press, Syracuse, N.Y., 1954. ix + 146 pp. Illus. $3.

THIS small readable book presents an objective and dispassionate discussion of elements of strength and weakness in the U.S.S.R. and of their significance by an outstanding authority on the geography of Asia. The author concludes that under any system of government this part of the world will never rate more than a B-plus or A-minus, because of its geographic handicaps, and that while "It is a serious mistake to overlook Russia's assets and the notable achievements of the Five Year Plans; it is equally a mistake to believe that the country holds material resources which might enable it to conquer the world."

The analysis is broad, well supported by citations of evidence, and presented with conviction. The conclusions are straightforward and categorical, in some instances perhaps unnecessarily categorical, for instance in the use of *never* and *always* in reference to the future, or the statement (p. 69) that the North American areas in the latitude of Moscow are beyond the limit of settlement. The point could sometimes have been made better with a qualification or two.

The conclusions also fit the present facts. How well they will fit future situations each reader will have to decide for himself. The author suggests that opinions on this will differ. I am less inclined than the author to discount the possibility that technologic progress may make usable some areas that at present have little or low productivity, or that further acquisitions of territory by one means or another may alter the picture in favor of the Soviet Union, but the basic handicaps, such as distance and recurrent drought, will still remain.

The production statistics cannot always be taken at face value, as Cressey cautions, but the information most important to this analysis—on distances, climate, vegetation, soils, and the nature and amount of resources—is not open to question to a significant extent. Erroneous statements are few, and the rare ones, such as that reindeer "feed chiefly on the sphagnum moss of the tundra," are inconsequential to the argument.

After quoting McKinder's observation that German technical and organizational skill combined with Russian resources would produce a formidable military power, the author might well have referred to the role of Germans captured or removed from satellite countries during or after World War II and to foreign technical help before or since that time. Information on this point is obviously difficult to obtain, but the results appear obvious and the consequences may be far-reaching.

The organization of the book involves considerable repetition. The reiteration of some points serves to keep them in continual focus, but it seems hardly necessary to repeat on pages 37, 93, 109, and 115 that the dam at Kuibyshev is the size of Grand Coulee Dam. On the whole, the style is clear, concise, and coherent, and the numerous and well-chosen illustrations include the maps necessary to follow the text and to visualize the space relationships.

This is an exceptionally interesting and forthright book that can help Americans to begin to understand, as few now do, the Soviet Union, its people, and the actions of its government. It deserves to be read widely, thoughtfully, and with an open mind. It will reinforce few prejudices already held. It may help to dispel some.

MEREDITH F. BURRILL

Division of Geography, U.S. Department of the Interior

The Flood Control Controversy. Big dams, little dams, and land management. Luna B. Leopold and Thomas Maddock, Jr. Ronald Press, New York, 1954. xiii + 278 pp. $5.

SPONSORED by the Conservation Foundation, this book is an impartial discussion of flood control and the argument between the proponents of big dams and the proponents of little dams. The authors, who are hydraulic engineers with years of experience in the field of water resources and land management, point out that flood damage has increased because industrial and agricultural expansion in the United States has been accompanied by greater use of lands subject to floods. Congress therefore charged the Corps of Engineers with the responsibility for Federal improvement of rivers and waterways. Congress also recognized the land-management aspect of the problem and made the Department of Agriculture responsible

for studies of the watersheds and for the adoption of measures to slow the runoff of storm water and consequently to reduce the size of floods.

The value of land management in the improvement of farm, range, and forest by conservation of soil resources, in the reduction of erosion, and in increasing agricultural yield is not a subject of controversy. Originally it was hoped that these management measures would do the major part of the upstream flood control job, but it was soon learned that such methods are insufficient to provide the desired degree of control over local floods. The Department of Agriculture, therefore, now includes in its plans much structural work, particularly flood-retarding reservoirs.

The over-all program of this department has had a considerable corrective effect on floods caused by local small storms, but the value of these measures decreases as the drainage area and the storm rainfall increase. These measures, therefore, are no substitute for big dams an main rivers. But the big dam obviously has no effect on floods upstream from it. The two programs are not interchangeable and each benefits persons and areas that are separated geographically.

Originally there was local financial participation in much of the flood-control effort, but today the Federal Government is underwriting the entire job. The authors show that we are unable to evaluate flood-control programs. They believe that the controversy over spending Federal flood-control funds will continue until local interests vying for these funds are willing to participate financially in proportion to the expected local benefit.

The book covers all the important aspects of flood control. It is scientific but very easy to read and understand. If it is widely read by those interested in the flocd-control problem, it should go a long way toward dispelling the darkness that at present shrouds this controversial subject.

WILLIAM H. ROWAN
Department of Civil Engineering, Vanderbilt University

A Thousand Geese. Peter Scott and James Fisher. Houghton Mifflin, Boston, 1954. 240 pp. Illus. + plates. $4.

THIS book comprises a running account of an expedition (1951) to find the principal breeding grounds of the pink-footed goose in the interior of Iceland. Most of these geese winter in England and Scotland. They are now known to breed in Spitzbergen, Greenland, and Iceland, but the last region is the breeding ground of the bulk of the population.

As the book progresses, one gets the "feel" of Iceland, at least of its cool, wet interior. It is an open land of sparse vegetation, of snow-fed streams, and of glacial moraines and icecaps. The photographs show this well when one can see past the horses, tents, or human beings that loom large in almost every picture. This impression will be the major contribution to the average reader. Although there is ornithologic information in the general text, it is so unusually diffuse that those interested will tire in the search through the interminable chitchat about such things as menus, sunbathing, clothes-drying, and radio reception. Even these topics would be less painful if they were well written. It seems that all diaries need extensive expunging and revision before being committed to type.

The appendixes constitute a valuable scientific publication. They relate the history of the discovery and the populations of pink-footed geese in the principal breeding areas. One appendix is concerned with the birds of the central oasis in Iceland; another (by Finnur Gudmundsson) deals with the plants found there. An index and a bibliography of about 80 titles complete the book.

The goose population in the area under observation was estimated at 13,000 in 1951 (5500 adults and 7500 goslings). Of these, 382 adults and 769 goslings were banded by the members of this expedition.

HARVEY I. FISHER
Department of Zoology, Uninversity of Illinois

Urban Behavior. E. Gordon Ericksen. Macmillan, New York, 1954. xiv + 482 pp. Illus. + plates. $4.75.

THE phenomenon of urbanization has had such tremendous impact on man's life that it has been subjected to inquiry by many disciplines. In this book the author has attempted to bring together some of these contributions.

The first part is a general discussion of the historical development of the city and the consequent theoretical speculations about rural-urban life.

The next section, which is the major portion of the book, is devoted to "The city as a physical mechanism —human ecology." Borrowing the concepts from plant and animal ecology, ecologists have attempted to show parallel behavior patterns in the urban community. This section includes a discussion of the geographic base of the city, the effects of technology on its location, ecologic processes, and urban land use.

The following section is concerned with the sociopsychological aspects of urbanization. As the author states,

> In the preceding chapters . . . we were concerned with *physical* space. . . . In this and the succeeding chapter we are concerned with *psychic* space; i.e., the urban personality and its relation to other men and groups.

Here Ericksen utilizes the findings in the behavioral sciences. Particularly emphasized is the paradox that, although city life has brought people closer together physically, it has not brought them together socially. This is illustrated by the diversity of values and the lack of consensus exhibited by urbanites.

Finally, the author calls attention to the possible role of urban planning in reallocating the functions within the city to meet best the demands for physical efficiency as well as for cultural and social adjustments.

The urban sociologist's main interest is the objective treatment of symbolic behavior with only implicit reference to geography. Here the author has covered the fundamental facts and principles of urban behavior. If in the process he has created more problems than he has solved, it is only because no one theory has adequately embraced all the pertinent factors that determine the spatial configurations of the city.

HEINZ J. GRAALFS
Sociology Department, University of Washington

Man, Time, and Fossils. The story of evolution. Ruth Moore. Knopf, New York, 1953. xvii + 424 pp. Illus. + plates. $5.75.

THIS could have been such a good book! It is attractively printed, bound, and illustrated. The theme, the history of evolutionary theories, is one that has been, if not neglected, at least given little attention on the popular level. The author writes interestingly and clearly on the whole, although she sometimes allows her enthusiasm to run away with her. The plan of the book—a series of chapters focused on the great individuals who have advanced evolutionary theory to its present position—is admirable for the purpose, a plan that allows the writer to include as much human interest as is necessary to appeal to a general reader. What then has gone amiss?

There is a lot of fairly accurate information in the book. There is also a lot of inaccuracy, ranging from minor slips to misleading errors. These are so numerous that it seems scarcely worth while to give examples—they run, on the average, about one or two per page. Take, for instance, page 6, introducing the Australopithecines: ". . . the missing link had at long last been found." What a bundle of misconceptions here! As though, in the first place, there was, or could be, a single missing link! As though the Australopithecines, important as they are, could be considered any more important than a number of others, *Pithecanthropus,* for instance! Worst of all, as though the Australopithecines were indeed on the direct line of human ancestry, a view now abandoned by most anthropologists. Or take the concluding passages of the book, where it is stated that "some of the Neanderthal men of Europe had a brain capacity very close to that of modern man, if not within the modern range"—a misleading slur upon those big-brained Paleolithic people that is quite at variance with the table given just above the statement. More serious is the logical error in the dictum that the new fluorine-dating methods applied by Oakley indicate that "humans who had the requisite intelligence to be called men did not reach that high status until about 50,000 years ago"—a *non sequitur* not borne out by the apparent fact that they *had* reached that status 50,000 years ago. Even in the list of references egregious errors occur. Julian Huxley's masterpiece, *Evolution the Modern Synthesis,* is said to be a "collection of essays by leading scientists," and Haldane is credited with one particular chapter. Perhaps Ruth Moore's book would have been better if she had studied *Evolution the Modern Synthesis* just a bit more closely.

The abundant errors are not, however, the most grievous flaw in this unfortunate best-seller. The crimes of omission and misplaced emphasis are much more serious. To some extent these are inherent in the author's choice of personages to whom chapters were to be devoted, for there are obvious gaps, from Lyell and Malthus to Dobzhansky, Simpson, Rensch, and Goldschmidt, and on the other hand some men are included who cannot be said to have contributed greatly to the store of either evolutionary theory or evidence. What can one say, for example, of a chapter devoted to Alfred Giard, whose almost sole claim to such attention is that he finally aroused the laggard biologists of France to a recognition of the importance of the theory of organic evolution? Considering certain other places where the author has shown favor to Lamarckian views, it is to be suspected that Giard is here to bolster that cause, although he scarcely forms a stout pillar.

Most serious of all is the enormous leap from Mendel and De Vries to Fisher, Wright, and Haldane. To ignore so completely the development of the chromosome theory of heredity and the role of the little fruitfly *Drosophila* in the development of modern evolutionary theory is simply incredible. The work of T. H. Morgan and his school, the discovery of the experimental production of mutation by x-rays and chemical agents, the production of artificial species, the studies of isolation mechanisms, both genetic and geographic, and their roles in the differentiation of races into species, and many other fundamental aspects of modern evolution theory have been completely overlooked, except in one ludicrous chapter in which Haldane's own contributions are virtually ignored while he appears to be given credit for everything he once discussed in his semi-popular book on the *Causes of Evolution.* It is hard to decide whether Haldane will be the more dismayed at this, or Fisher when the latter sees the unbelievably naive and meaningless treatment of his mathematics on page 193.

Other reviewers have been somewhat kinder to this book. For my part, I think the illustrations are worth saving.

BENTLEY GLASS
Department of Biology, Johns Hopkins University

The Mating Instinct. Lorus J. and Margery J. Milne. Little, Brown, Boston, 1954. 243 pp. Illus. $4.50.

IT is only within the last decade or two that books on sexual matters could be written for others than the medical and allied professions. Taboos and prejudices have gradually lessened, and in the last few years we have seen a succession of books on sexual behavior in both animals and human beings, written for the professional and layman alike. Although certain of these books have raised a plethora of criticism, particularly with regard to methodology and interpretation, still it is not difficult to agree that all are works of consider-

able merit. To this notable series, the Milne team now make their contribution.

The Mating Instinct is written primarily for the layman and amateur naturalist. It is in essence a huge assemblage of items, interesting, odd, unique, or exciting, and running the entire gamut of the animal kingdom. The subject of sexual behavior is considered in its broadest sense. Thus there are chapters on the differences or similarities between males and females, discrimination of the sexes, pursuit, combat, courtship, mating, egg laying and birth, parental behavior, and genetic determination of sex; the book ends with a straightforward and sound discussion of the adaptive value of sex in terms of modern evolutionary theory. In all the chapters except the last two, one encounters a continuous barrage of biological facts, presented interestingly enough, but nevertheless with little attempt at organization and with no unifying generalizations other than the magnitude, diversity, and wondrousness of nature. In the process of assorting and assembling a large collection of items such as these, it is next to impossible to eliminate every last error. However, because of a number of glaring misstatements, it appears that the Milnes exceeded this irreducible minimum by a considerable degree.

The term *instinct* is a key word in the title, and it is repeated dozens of times throughout the book, often included in mystifying expressions such as "automatic instinct," "close to being an instinct," "instinctive purpose," and so forth. These expressions are never once defined or discussed by the authors, who seem to imply, in a vague and misleading way, that the particular items of behavior so characterized are solely the products of our heredity, and that they arise as an inner animating force, independent of the animals' environment and experiences. Concepts of this nature certainly lack clarity and accuracy of scientific thought, as has been pointed out at various times in recent years by American psychologists. Their adoption and persistent emphasis by the present authors only distract from an otherwise interesting and entertaining book.

LESTER R. ARONSON
Department of Animal Behavior,
American Museum of Natural History

So Little for the Mind. Hilda Neatby. Clarke, Irwin, Toronto, ed. 2, 1953. xiii + 384 pp. $3.

THIS is a first-rate book: a devastating analysis of the theory and practice of education in the English-language schools of Canada's nine older provinces. Hilda Neatby, professor of history in the University of Saskatchewan and a member of the Royal Commission on National Development in the Arts, Letters and Sciences, knows what she is talking about and how to say it. The result is a scholarly work which is also a popular triumph. First published in October 1953, it has gone through two editions and five printings in 7 months, something hitherto unheard of, and indeed undreamed of, for a "serious" book in Canada.

There are four main reasons for this. First, the book is superbly written: every page sparkles. Second, the indictment is supported by ample evidence, mostly direct quotations from the "educational experts" themselves. Third, it has produced an unprecedented uproar from the "experts" in question, who naturally have not enjoyed seeing themselves convicted, out of their own mouths, of ignorance, arrogance, blandly unconscious totalitarianism, and an almost unbelievable incapacity to write plain English. Fourth, the ensuing discussion has shown that Neatby was only saying aloud, trenchantly, and with a wealth of evidence to support it, what many parents, university teachers, and employers had been feeling for some time. A lot of the consumers of the products of Canadian schools are fed up, but most of them, until now, have been afraid to say so. Hilda Neatby has given them courage and a reason for the faith that is in them, and they have rallied to her with enthusiastic gratitude.

The "great debate" to which this book is so outstanding a contribution is raging on both sides of the border, and American readers will probably find that much of what the author has to say is as relevant in the United States as it is in Canada. They will find no neat prescriptions for the ills that beset education on this continent. Hilda Neatby is too modest to think that she has all the answers. But they will find a brilliant diagnosis and a clear statement of the lines along which Hilda Neatby thinks the answers will be found. The spirit of the whole is summed up in the last sentence:

> The present preoccupations with body building and character moulding are useless and may even be dangerous so long as we neglect and starve the mind.

EUGENE FORSEY
Director of Research, Canadian Congress of Labour

Educational Psychology. Lee J. Cronbach. Harcourt, Brace, New York, 1954. xxvii + 628 pp. Trade edition, $7.50; text edition, $5.50.

THIS new *Educational Psychology* is divided in five main sections: "Psychology and school problems"; "Readiness and its development"; "Acquiring ideas, attitudes, and skills"; "Planning, motivation, and evaluation"; and "Emotional learning."

It is amazing how much of the total field of psychology really belongs in a well-balanced book dealing with the learning and teaching processes. In addition to techniques and effectiveness of learning and memory (the standbys of traditional textbooks on the subject), we find all the following points. Intelligence and other aptitudes must be studied to pitch subject-matter properly and to understand individual differences in rates and capacities to learn. We must know child psychology to understand not only learning but motivation, emotions, personality formation, and the quirks caused by different homes and parents, physical differences, and the particular talents of the boy or girl. Then too, reasoning and problem solving, vision and reading, mental hygiene, and tests and their interpretation all have a place.

There is ample use of case histories, given in enough detail to supply full meaning, and plainly interpreted right in the body of the case so one can use the author's suggestions for handling parallel cases which are bound to arise sooner or later in any teacher's experience.

I sighed with relief at two observations. First, while there is ample use of illustrations, both photograph and cartoon drawings, these are pertinent to the accompanying text, not just inserted as Sunday-supplement attention-getting devices. Second, Cronbach is to be congratulated for refraining from pontificating *ad nauseam* what is too often found in textbooks on education, namely, the role and duty of the teacher to interpret the American way of life, democracy, and the atomic age to the pupils.

Here, in my opinion, is a new textbook that should gain wide acceptance and usage, not only for formal class adoption but for use by individual teachers to improve their techniques—college teachers included. Cronbach is really applying scientific psychology to the classroom. Principles and research findings are applied logically, and not, as we often see, rather obliquely and dragged in by the heels. Chapter 15 alone, "The teacher as classroom leader," is worth the price of the book.

RICHARD W. HUSBAND
Department of Psychology, Florida State University

Applied Atomic Energy. K. Fearnside, E. W. Jones, and E. N. Shaw. Philosophical Library, New York, 1954. viii + 156 pp. Illus. + plates. $4.75.

THIS book is intended to summarize for the nonnuclear scientists and engineers the uses of radioactive isotopes. The book is naturally divided into two major sections. The first part deals with a superficial background in nuclear physics, nuclear reactors, and isotope preparation methods; the second is devoted to the techniques and uses of radioisotopes in various fields of endeavor.

It seems clear to me that the authors have considerably more experience in isotope application and use than in the fundamentals of nuclear physics and reactor technology. The early chapters contain many inaccuracies; some may be misprints but others indicate a lack of familiarity with the scientific foundation of the nuclear energy business. It is unfortunate that the authors have attempted to include this material in such a book, because the lack of sophistication in these early chapters prejudices the reader into believing that their judgment in other matters may be equally unsound.

The chapters on application cover the use of radioisotopes in biology, medicine, agriculture, and industry. This section is largely a catalog of what has been done with radioisotopes to date in these fields. There are no serious inaccuracies in this section. On the whole, the chapters are too disjointed to make good reading or to supply a strong background preparatory to using radioisotopes in any practical way.

It is unfortunate that this book was not written in a more knowledgeable fashion, since it is clear that there is a current need for a good textbook in this field. However, it falls short of this mark, and I cannot recommend it to anyone seriously interested in the application of radioisotopes to either industrial or research problems.

ROBERT A. CHARPIE
Oak Ridge National Laboratory

Fifty Years of Medicine. Lord Horder. Philosophical Library, New York, 1954. 70 pp. $2.50.

THIS slim volume is an expanded version of the Harben lectures delivered at the Royal Institute of Public Health and Hygiene in December, 1952, by one of England's most eminent and distinguished medical statesmen. Within the space of less than 100 pages the author has boldly, brilliantly, and provocatively outlined the developments in medicine during the last 50 years. Because Horder has lived through the history he recounts and has, in fact, been one of the artisans of that history, his account has a special interest. In three lectures Horder reviews the birth of scientific medicine and how medicine has enlarged its boundaries and takes a look at the present and future.

Among the topics touched on with authority, with humor, and sometimes with lively heterodoxy, but always with deep humanity, are chemotherapy, the birth of clinical pathology, advances in surgery, the rise of social medicine, industrial medicine, physical medicine and rehabilitation, medicine and the state, and the diseases of war.

This is a small but meaty book, and anyone interested in the broad problems of medicine and society will find that an evening spent reading it will be an evening well spent.

MORRIS C. LEIKIND
Armed Forces Institute of Pathology

Engineers' Dreams. Willy Ley. Viking Press, New York, 1954. 239 pp. Illus. $3.50.

THE history of science and engineering is the record of man's dreams, ranging from the semiconscious fantasy of Kekulé leading to the concept of the benzene ring to the very tangible drafting boards for a new bridge.

All engineering and scientific developments started with someone's dream that something could be done or that some improvement could be made on a familiar art. The finished products of dreams, speculations, and work are the final achievements of our modern world of science and technology. But what about the dreams that failed just short of realization, the dreams that needed more funds or more dreamers to push them to completion? It is about a few such dreams that Ley writes in this book. He indulges himself and fascinates his readers with the story of projects in power and transportation engineering that almost made the grade.

A tunnel between France and England would aid

transportation and communication. How many people know that it has been started in both countries and may even yet be finished? Floating islands in the oceans for air transport would be with us now if the efficiency of planes had not developed faster than the plans and funds for the artificial islands.

Power is the magic word that makes our civilization possible, and someday it may be necessary to supplement our resources with the projects called engineers' dreams today, but which might be tomorrow's reality. The damming of the Jordan Valley, the utilization of volcanic heat, the use of sunlight, and harnessing the winds and the waves are all projects on which able men have spent their thoughts and lives. Some of the projects have been partially utilized, such as volcanic heat for power in Italy, but most volcanoes still waste their power potentials. The blockage of the Congo basin and the Mediterranean Sea would certainly open up new lands, give power at some points, and affect the climate of the neighboring countries. To the obvious engineering difficulties of some of these projects must be added the political obstacles—one is tempted to say "power politics"!

The amazing thing about this book is that it cannot be considered fantasy, because all the projects discussed can be carried out if sufficient money, effort, and political agreement can be obtained. How many will finally be realized is another question. However, some of the projects are on the way now.

Engineers' Dreams has been classified as a "juvenile" for trade purposes. It immediately won a prize in the *New York Herald Tribune* Children's Spring Book Festival, 1954. Boys and girls will undoubtedly enjoy this fascinating book whenever they can get it away from their fathers and mothers who will probably use their parental privilege of reading it first.

THOMAS S. GARDNER

Rutherford, New Jersey

Books Reviewed in SCIENCE

5 November

Snow Crystals: Natural and Artificial, Ukichiro Nakaya (Harvard Univ. Press). Reviewed by H. B. Nichols.

Chemistry of the Defect Solid State, A. L. G. Rees (Methuen; Wiley). Reviewed by J. M. Honig.

Thomas Young, Natural Philosopher: 1773–1829. The late Alexander Wood; completed by Frank Oldham (Cambridge Univ. Press). Reviewed by R. B. Lindsay.

Energy Transfer in Hot Gases, Proceedings of NBS Symposium 17–18 Sept. 1951 (Natl. Bur. of Standards). Reviewed by M. Jakob.

Essays on the Social History of Science, S. Lilley, Ed. (Munksgaard). Reviewed by M. C. Leikind.

Elements of Statistical Mechanics, D. ter Haar (Rinehart). Reviewed by J. E. Mayer.

Histology, Roy O. Greep, Ed. (Blakiston). Reviewed by E. B. Scott.

12 November

Graphics in Engineering and Science, A. S. Levens (Wiley; Chapman & Hall). Reviewed by A. N. Appleby.

The Genus Euglena, Mary Gojdics (Univ. of Wisconsin Press). Reviewed by L. P. Johnson.

Artificial Fibres, R. W. Moncrieff (Wiley). Reviewed by F. J. Soday.

Diagnosis and Improvement of Saline and Alkali Soils, L. A. Richards, Ed. (U. S. Dept. of Agriculture). Reviewed by J. C. Hide.

Catalysis, vol. I, pt. 1, Paul H. Emmett, Ed. (Reinhold). Reviewed by V. K. LaMer.

19 November

Vegetable Fats and Oils, E. W. Eckey (Reinhold). Reviewed by C. Artom.

Metabolism of Steroid Hormones, Ralph I. Dorfman and Frank Ungar (Burgess). Reviewed by L. T. Samuels.

Qualitative Analysis and Chemical Equilibrium, T. R. Hogness and Warren C. Johnson (Holt). Reviewed by N. H. Furman.

Representative Chordates, Charles K. Weichert (McGraw-Hill). Reviewed by W. L. Straus, Jr.

The Properties of Glass, George W. Morey (Reinhold). Reviewed by C. L. Babcock.

The Steel Skeleton, vol. I, J. F. Baker (Cambridge Univ. Press). Reviewed by F. Baron.

Biographical Memoirs, vol. XXVIII (Columbia Univ. Press). Reviewed by J. Oppenheimer.

Coronary Heart Disease in Young Adults, Menard M. Gertler and Paul D. White (Harvard Univ. Press). Reviewed by A. A. Luisada.

Vegetable Tanning Materials, F. N. Howes (Chronica Botanica; Butterworths). Reviewed by T. G. Greaves.

Crystal Data, J. D. H. Donnay and Werner Nowacki (Geological Society of America). Reviewed by A. L. Patterson.

26 November

The Optical Properties of Organic Compounds, Alexander N. Winchell (Academic Press). Reviewed by M. L. Willard.

Tissue Culture, E. N. Willmer (Methuen; Wiley). Reviewed by G. O. Gey.

A Textbook of Radar, E. G. Bowen, Ed. (Cambridge Univ. Press). Reviewed by L. W. Alvarez.

Handbuch der Pflanzenkrankheiten, vol. II, E. Kohler, M. Klinkowski, O. Appel, H. Richter, Eds. (Parey). Reviewed by C. Chupp.

Physiology and Biochemistry of the Skin, Stephen Rothman (Univ. of Chicago Press). Reviewed by D. H. K. Lee.

Yeast Technology, John White (Wiley). Reviewed by C. L. Brooke.

Gmelins Handbuch der Organischen Chemie: Schwefel (Sulfur), Sec. A-2, A-3, B-I (Walter J. Johnson; Stechert-Hafner). Reviewed by R. Gilchrist.

For a Science of Social Man, John Gillin, Ed. (Macmillan). Reviewed by C. Kluckhohn.

The Biochemistry of Clinical Medicine, W. S. Hoffman (Year Book Pub.). Reviewed by R. S. Goodhart.

New Books

Rural Electrification. vol. I, ii + 163 pp. + tables. Plates. Paper, $1.50. vol. II, ii + 165 pp. Plates. Paper, $1.25. United Nations Economic and Social Council, Geneva, Switzerland, 1954 (U.S. Distrib.: Columbia Univ. Press, New York).

Zoology. Clarence J. Goodnight and Marie L. Goodnight. Mosby, St. Louis, 1954. 730 pp. Illus. $6.50.

Reviews of Research on Problems of Utilization of Saline Water. Arid Zone Programme, No. IV. UNESCO, Paris, 1954 (Order from Columbia Univ. Press, New York 27). 96 pp. Illus. Paper, $1.75.

A History of Mechanical Inventions. Abbott Payson Usher. Harvard Univ. Press, Cambridge, Mass., rev. ed., 1954. xi + 450 pp. Illus. $9.

Metabolic Interrelations. With special reference to calcium. Trans. of the Fifth Conference, 5–6 Jan. 1953. Edward C. Reifenstein, Jr., Ed. Josiah Macy, Jr. Foundation, New York, 1954. 386 pp. Illus. $5.

A Devotion to Nutrition. Frederick Hoelzel. Vantage Press, New York, 1954. 88 pp. $2.50.

Elements of Food Engineering. vol. 3, ***Unit Operations***, pt. 2. Milton E. Parker; with the collaboration of Ellery H. Harvey and E. S. Stateler. Reinhold, New York, 1954. 241 pp. Illus. $6.75.

The Rise and Fall of Maya Civilization. J. Eric S. Thompson. Univ. of Oklahoma Press, Norman, 1954. xii + 287 pp. Illus. + plates. $5.

Fiberglas Reinforced Plastics. Ralph H. Sonneborn; with chapters by Albert G. H. Dietz and Alton S. Heyser. Reinhold, New York, 1954. xii + 244 pp. Illus. $4.50.

Chemical Pathways of Metabolism. vol. II. David M. Greenberg, Ed. Academic Press, New York, 1954. vii + 383 pp. Illus. $9.50.

Qualitative Analysis Using Semimicro Methods. Esmarch S. Gilreath. McGraw-Hill, New York, 1954. viii + 287 pp. Illus. $4.25.

Le Magnétisme des Corps Célestes. A. Dauvillier. vol. I, *Magnétisme Solaire et Stellaire Couronne Solaire et Lumière Zodiacale.* 171 pp. Illus. + plates. Paper, F. 1600. vol. 2, *Variations et Origine du Géomagnétisme.* 161 pp. Illus. + plates. Paper, F. 1500. Hermann, Paris, 1954.

Highway Engineering. Laurence I. Hewes and Clarkson H. Oglesby. Wiley, New York; Chapman & Hall, London, 1954. xi + 628 pp. Illus. $8.

Dictionary of Linguistics. Mario A. Pei and Frank Gaynor. Philosophical Library, New York, 1954. 238 pp. $6.

Successful Commercial Chemical Development. H. M. Corley, Ed. Wiley, New York; Chapman & Hall, London, 1954. xxv + 374 pp. $7.75.

Amphibians and Reptiles of Western North America. Robert C. Stebbins. McGraw-Hill, New York, 1954. xxii + 528 pp. Illus. + plates. $8.50.

God and Space-Time. Deity in the philosophy of Samuel Alexander. Alfred P. Stiernotte. Philosophical Library, New York, 1954. xxvii + 455 pp. $5.

Protozoology. Richard R. Kudo. Charles C. Thomas, Springfield, Ill., ed. 4, 1954. xi + 966 pp. Illus. $10.75.

Indians of the Plains. American Museum of Natural History Anthropological Handbook No. 1. Robert H. Lowie. McGraw-Hill, New York, 1954. xiii + 222 pp. Illus. $4.75.

Mathematical Puzzles and Pastimes. Aaron Bakst. Van Nostrand, New York–Toronto; Macmillan, London, 1954. vi + 206 pp. Illus. $3.75.

The Vitamins: Chemistry, Physiology, Pathology. vol. II. W. H. Sebrell, Jr., and Robert S. Harris, Eds. Academic Press, New York, 1954. xiii + 766 pp. Illus. $16.50.

Diagrams in Punched Card Computing. Fred Gruenberger. Univ. of Wisconsin Press, Madison, 1954. ix + 108 pp. Illus. Paper, $3.75.

Plant Regulators in Agriculture. H. B. Tukey, Ed. Wiley, New York; Chapman & Hall, London, 1954. x + 269 pp. Illus. $5.50.

Annual Report of the Board of Regents of the Smithsonian Institution. Showing the operations, expenditures, and condition of the Institution for the year ended 30 June 1953. Pub. 4149, 1954 (Order from Supt. of Documents, GPO, Washington 25). ix + 481 pp. Illus. + plates $3.75.

The Design and Use of Instruments and Accurate Mechanism. Underlying principles. T. N. Whitehead. Dover, New York, printing 2, 1954. xiv + 283 pp. Illus. Paper, $1.95; cloth, $3.50.

Exploring Our National Parks and Monuments. Devereux Butcher. Houghton Mifflin, Boston, ed. 4, 1954. viii + 288 pp. Illus. $4.50.

Plastics Engineering Handbook. Society of the Plastics Industry. Reinhold, New York, ed. 2, 1954. xxxv + 813 pp. Illus. $15.

Teen-Agers. A health and personal development text for all teen-agers. Gladys Gardner Jenkins, W. W. Bauer, and Helen S. Shacter. Scott, Foresman, Chicago, 1954. 288 pp. Illus. $3.60.

Bibliography and Index of Geology Exclusive of North America. vol. 18. Marie Siegrist, Mary C. Grier, and Marcia Lakeman. Geological Society of America, New York, 1954. xiv + 599 pp.

Readings in General Psychology. Lester D. Crow and Alice Crow. Barnes & Noble, New York, 1954. x + 437 pp. Illus. Paper, $1.75.

Monomeric Acrylic Esters. Edward H. Riddle. Reinhold, New York, 1954. vii + 221 pp. Illus. $5.

Men of Other Planets. Kenneth Heuer. Viking, New York, reissue, 1954 (Pellegrini & Cudahy, New York 1951). ix + 160 pp. Illus. $3.

Active Networks. Vincent C. Rideout. Prentice-Hall, New York, 1954. xvi + 485 pp. Illus. $10.65.

Collections of a Century. The history of the first hundred years of the National Museum of Victoria. R. T. M. Pescott. National Museum of Victoria, Melbourne, 1954. x + 186 pp. Illus.

Practical Physiological Chemistry. Philip B. Hawk, Bernard L. Oser, and William H. Summerson. Blakiston, New York, ed. 13, 1954. xvi + 1439 pp. Illus. $12.

An Introduction to the Study of Fossil Plants. John Walton. Black, London, ed. 2, 1953 (U. S. distr.: Macmillan, New York). x + 201 pp. Illus. $4.75.

Explaining the Atom. Selig Hecht. Rev. by Eugene Rabinowitch. Viking, New York, 1954. xviii + 237 pp. Illus. $3.75.

Art of Asia. Helen Rubissow. Philosophical Library, New York, 1954. xiii + 237 pp. Illus. $6.

The ABC of Natural Childbirth. Barbara Gelb. Norton, New York, 1954. 190 pp. Illus. $2.95.

Biochemical Determinants of Microbial Diseases. René J. Dubos. Harvard Univ. Press, Cambridge, 1954. viii + 152 pp. $3.50.

Heterocyclic Compounds with Indole and Carbazole Systems. Ward C. Sumpter and F. M. Miller. vol. VIII of *The Chemistry of Heterocyclic Compounds*, Arnold Weissberger, Ed. Interscience, New York–London, 1954. xii + 307 pp. Single copy, $10; subscription, $9.

International Review of Cytology. vol. III. G. H. Bourne and J. F. Danielli, Eds. Academic Press, New York, 1954. 530 pp. Illus. $9.50.

Decision Processes. R. M. Thrall, C. H. Coombs, and R. L. Davis, Eds. Wiley, New York; Chapman & Hall, London, 1954. viii + 332 pp. Illus. $5.

Concepts of Space. The history of theories of space in physics. Max Jammer. Harvard Univ. Press, Cambridge, Mass., 1954. xvi + 196 pp. $3.75.

Clinical Aspects of the Autonomic Nervous System. L. A. Gillilan. Little, Brown, Boston, 1954. xii + 316 pp. Illus. $6.50.

Annual Review of Physical Chemistry. vol. 5. G. K. Rollefson and R. E. Powell, Eds. Annual Reviews, Stanford, Calif., 1954. ix + 540 pp. Illus. $7.

Administrative Medicine. Trans. of the 2nd conference, 8–10 Dec. 1953. George S. Stevenson, Ed. Josiah Macy, Jr. Foundation, New York, 1954. 164 pp. $3.

Coro-Coro. The world of the scarlet ibis. Paul A. Zahl. Bobbs-Merrill, Indianapolis, Ind., 1954. xv + 264 pp. Plates. $4.50.

Cellulose and Cellulose Derivatives. pt. I. Emil Ott, Harold M. Spurlin, and Mildred W. Grafflin, Eds. Interscience, New York–London, ed. 2, 1954. xvi + 509 pp. Illus. $12.

The New Warfare. C. N. Barclay. Philosophical Library, New York, 1954. x + 65 pp. $2.75.

New Instrumental Methods in Electrochemistry. Theory, instrumentation, and applications to analytical and physical chemistry. Paul Delahay. Interscience, New York–London, 1954. xvii + 437 pp. Illus. $11.50.

Modern Physics for the Engineer. Louis N. Ridenour, Ed. McGraw-Hill, New York–London, 1954. xix + 499 pp. Illus. $7.50.

Wool: Its Chemistry and Physics. Peter Alexander and Robert F. Hudson. Chapman & Hall, London; Reinhold, New York, 1954. viii + 404 pp. Illus. + plates. $10.

Trigonometry. Elbridge P. Vance. Addison-Wesley, Cambridge, Mass., 1954. viii + 158 pp. Illus. $3.

Symposium on Problems of Gerontology. Nutrition Symposium Ser., No. 9. Robert S. Goodhart, Ed. National Vitamin Foundation, New York, 1954. ii + 141 pp. Illus. Paper, $2.50.

The Strengths of Chemical Bonds. T. L. Cottrell. Academic Press, New York; Butterworths, London, 1954. viii + 310 pp. Illus. $5.50.

Strength and Resistance of Metals. John M. Lessells. Wiley, New York; Chapman & Hall, London, 1954. xiv + 450 pp. Illus. $10.

Statistics in Research. Basic concepts and techniques for research workers. Bernard Ostle. Iowa State College Press, Ames, 1954. xiv + 487 pp. $6.95.

Sovereign Reason. And other studies in the philosophy of science. Ernest Nagel. Free Press, Glencoe, Ill., 1954. 315 pp. $5.

Relative Chronologies in Old World Archeology. Robert W. Ehrich, Ed. Univ. of Chicago Press, Chicago, 1954. xiii + 154 pp. Illus. Paper, $2.50.

Meetings

January

13. American Genetic Assoc., annual business, Washington, D.C. (R. M. Cook, AGA, 1507 M St., NW, Washington.)

18–20. Weight Control Colloquium, Iowa State College, Ames. (C. R. Elder, Morrill Hall, Iowa State College.)

21. Public Health Workshop on Dental Programs in Industry, 3rd, New York, N.Y. (A. J. Asgis, Hotel Statler, New York.)

24–27. American Soc. of Heating and Ventilating Engineers, Philadelphia, Pa. (A. V. Hutchinson, 62 Worth St., New York 13.)

26–2. Australian and New Zealand Assoc. for the Advancement of Science, Melbourne, Australia. (J. R. A. McMillan, 157 Gloucester St., Sydney.)

27. American Federation for Clinical Research, western section, Carmel, Calif. (A. B. French, College of Medicine, Univ. of Utah, Salt Lake City.)

27–28. Symposium on Hydrocarbon Chemistry, Houston, Tex. (L. B. Odell, Rice Hotel, P.O. Box 38, Houston.)

27–29. American Assoc. of Physics Teachers, New York, N.Y. (R. F. Paton, Dept. of Physics, Univ. of Illinois, Urbana.)

27–29. American Physical Soc., New York, N.Y. (K. K. Darrow, Columbia Univ., New York 27.)

28–29. Assoc. of Geology Teachers, Annual, Columbus, O. (Richard P. Goldthwait, Ohio State Univ., Columbus 10.)

February

4–5. American Geophysical Union, Berkeley, Calif. (D. K. Todd, College of Engineering, University of California, Berkeley 4.)

7–9. Conf. on Silicosis and Occupational Chest Diseases, Saranac Lake, N.Y. (N. R. Sturgis, Jr., Saranac Laboratory, Saranac Lake.)

7–11. American Soc. of Civil Engineers, San Diego, Calif. (W. N. Carey, 33 W. 39 St., New York 18.)

8–9. Midwest Welding Conf., Illinois Inst. of Technology, Chicago. (Mel Garbar, Illinois Inst. of Technology, 35 W. 33rd St., Chicago 16.)

11–25. Pan American Acad. of General Practice, Lima, Peru. (A. Martinez, 54 E. 72 St., New York 21.)

13–17. American Inst. of Mining and Metallurgical Engineers, annual, Chicago, Ill. (E. H. Robie, 29 W. 39 St., New York 18.)

14–16. Nutrition of Plants, Animals, Man. Michigan State College, Centennial Symposium, East Lansing. (Continuing Education Service, Kellogg Center, Michigan State College.)

14–19. Latin American Cong. of Physical Medicine, Lima, Peru. (C. L. de Victoria, 176 E. 71 St., New York 21.)

17–18. National Conf. on Transistor Circuits, Philadelphia, Pa. (W. J. Popowski, Minneapolis-Honeywell Regulator Co., 176 West Loudon St., Philadelphia 20.)

17–19. American Acad. of Forensic Sciences, Los Angeles, Calif. (W. J. R. Camp, 1853 Polk St., Chicago 12, Ill.)

28–1. American Orthopsychiatric Assoc., 32nd annual, Chicago, Ill. (Marion F. Langer, AOA, 1790 Broadway, New York 19.)

28–2. American Educational Research Assoc., St. Louis, Mo. (F. W. Hubbard, 1201 16 St., NW, Washington 6, D.C.)